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OBITUARY NOTICES OF FELLOWS DECEASED.

Dr. GEORGE BUDD was born in 1808. He was the third son of Mr. Samuel Budd, surgeon, of North Tawton, in Devonshire, and one of nine brothers, seven of whom have successfully studied and practised medicine. Having feeble health in early life, he was educated at home till he went to Cambridge in 1827.

In his first year he was at St. John's, afterwards at Caius', and in both colleges he lived a quiet studious life, reading hard, though often interrupted by illness. He was third wrangler in a good year (1831), and was soon elected to a fellowship in his college. He studied medicine at the Middlesex Hospital; was elected a Fellow of the Royal Society in 1836; took his degree of M.D. in 1848; and soon after became a Fellow of the Royal College of Physicians. His first publication was an ingenious essay on the Stethoscope as an Acoustic Instrument, in the "Medical Gazette" of 1837. It brought him good repute, and in the same year he was appointed Physician to the "Dreadnought" hospital ship, an office which he desired chiefly because it gave better opportunities for thorough pathological study than could, at that time, be found in any but the largest hospitals in London. It was here that, in association with Mr. Busk, he made his researches on Cholera and Scurvy, and collected the greater part of the pathological facts on which he based his later works on the Diseases of the Liver and of the Stomach.

Of the papers on Cholera, the first, in the "Medico-Chirurgical Transactions" (vol. xxi), by himself and Mr. Busk, is a report on cases in the "Dreadnought" during the epidemic of 1837; the second (in vol. xxii) a statistical account of cases collected from the records of the same hospital in 1832. They are among the best writings on the Cholera in this country; and the second, especially, is a good instance of the influence of the teaching of Louis, whose "numerical method," then hardly appreciated, had so great a share in promoting accuracy in medical research. Their chief results are collected in his essay on Cholera in "Tweedie's Library of Practical Medicine" (vol. iv), which also contains (in vol. v) his admirable essay on Scurvy, with Mr. Busk's observations on the condition of the blood in that disease.

In the same year (1837-8), and in the same Transactions, Dr. Budd published papers on Concentric Hypertrophy of the Heart and on Emphysema of the Lungs. In the former he established and applied Cruveilheir's opinion that this condition of hearts is characteristic of those "which death has surprised in all their energy of contractility." In the latter, he demonstrated the effects of the lungs' loss of elasticity, and, from experiments made with Mr. Busk, showed the inactivity of the tracheal and bronchial muscular fibres under galvanic stimulus, and thence argued against the too often assumed influence of the muscular structure of the air-tubes in the phenomena of asthma.

Each of these essays shows great habitual care in studying disease, not only in its own signs, but with constant reference to the best knowledge of the natural structure and action of the affected parts. The same method is evident in a paper in the forty-second volume of the "Medico-Chirurgical Transactions," in which he showed that the inflammatory changes in the lungs, in cases of primary cancer within the chest, are very probably due to the morbid growths involving the pulmonary nerve-plexuses, and thus producing effects similar to those produced by injury to the trunk or branches of the fifth cerebral nerve.

The same good method of study is shown on a much larger scale, and with a vast amount of careful observation and research, in Dr. Budd's chief work, his Treatise on Diseases of the Liver, published in 1845. Writing of this, Dr. Wilson Fox, who may be deemed the most capable judge, says, "He may fairly be said to be the first writer who, for nearly half a century, had systematised the practical knowledge of liver-diseases; and he, for the first time, gave this knowledge the form which it has retained for nearly forty years. This he did through the fact that he impressed on nearly every statement his own careful clinical observation, and reinvestigated the pathology of the subject in the light of the then recent anatomical works of Kiernan and Bowman. The result has been that his book remains and must remain an original work of the highest value, and marking a period."

Of scarcely less value was the Treatise on Diseases of the Stomach, published in 1855. It abounds in valuable clinical observations and cases carefully recorded, including those by which he showed that the self-digestion of the stomach after death is often due to specially corrosive secretions, whose action during life is prevented by the alkalinity of the circulating blood. His arguments on this subject show singularly clear and ingenious thought; just as do those with which, in his treatise on the liver, he proved that hepatic abscesses are very often due, not only to phlebitis of the minutest portal veins, but to blood-poisoning from diseased intestines, and criticised Claude Bernard's hepatogenic theory of diabetes in the presence of this disease with a degree of cirrhosis which had almost destroyed the structure of the liver.*

In 1848 Dr. Budd succeeded Sir Thomas Watson as Professor of Medicine in King's College, and was appointed Physician to its Hospital. He held this office for more than twenty years, and was actively engaged in teaching and with increasing private practice. He was a clear, emphatic, and persuasive lecturer, and in his clinical work, in both private and hospital practice, showed excellent examples of patient, exacting, and complete inquiry, and of careful study.

In 1866, with failing health, he retired from active life, and, from that time to his death, in March, 1882, lived quietly and studiously among his friends in Devonshire. The influence of his admirable personal character, and of his constancy in the patient, thoughtful striving for exact knowledge, will long survive among his many friends and pupils. In the history of medical science he will worthily rank with his brother, William Budd, a distinguished Fellow of the Society, to whom we owe, among many other good works, the first and best observations on the transmission of the typhoid infection in the intestinal excreta, and on the analogy between tuberculosis and infective fevers.—J. P.

JOSEPH DECAISNE, though so long a prominent figure in the French scientific world, was by birth a Belgian, having been born in Brussels, March 11, 1807. His brother, who survives him, is Honorary Inspector-General of the Belgian Army Medical Service. Decaisne entered the Jardin des Plantes, where the whole of his subsequent life was to be spent, as a gardener, at the early age of seventeen. He made his first contribution to botanical literature in 1831, in a paper on the characters of the French species of *Herniaria;* in 1840 he was attached to the Herbarium as *aide naturaliste*, finally returning to the Garden as *Professeur de Culture* and *Directeur* in succession to Mirbel. The half-century from Mirbel to the present day covers our whole modern knowledge of the histology and morphology of plants. The demonstrations of our class-rooms already seem a little commonplace; yet they deal with structures and phenomena which, when Decaisne first began to work, were things undreamt of.

In 1837 he published a memoir on the anatomy and physiology of the madder-plant and the development of its colouring matter. This was an excellent piece of work, remarkable at its time as an example of detailed study of the structure and life-history of an individual plant. At a very early period he turned his attention to the serious study of *Algæ*, and it is perhaps in connexion with this group that he has left his most indelible mark in botanical science. In 1841 he showed once for all that the *Polypiers calcifères* of Lamouroux were not merely *Algæ*, but that the affinities of the diverse types which they comprised could be determined with some certainty. This conclusion was not a happy guess, but was based on a laborious examination of the whole class of *Algæ* with the object of arranging their chaotic assemblage on a basis approaching as nearly as possible to a natural classification. The results were given in an elaborate paper, published in 1842. The divisions proposed are not essentially very different from those which are generally accepted at the present day; and they were really more natural than the subsequent but far more artificial classification proposed by Harvey, which has long held its ground in this country.

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* Besides the above-named works Dr. Budd published in journals, chiefly in the "Medical Gazette," several short papers and lectures, including the Gulstonian and Croonian Lectures delivered at the College of Physicians in 1843 and 1847.

In this particular line Decaisne himself, owing probably to the distraction of his energy by official duties, did little more. Notwithstanding this, he must always be regarded as the founder of the French school of Algology, the literature of which is the basis of our present knowledge of this branch of vegetable morphology. In 1839 Thuret came to Paris, and received from Decaisne instruction in the rudiments of botany. Decaisne and Thuret began to work together on the reproduction of *Fucus*, which they procured from the fish-market of Paris. They soon found it, however, necessary to visit the coast to carry on their observations, the results of which were published in 1844 in a joint paper in which they first accurately described the antherozoids, assigning to them their true function, and gave an account in some of the species of the beautiful process of the division of the primary oosphere. After Decaisne's appointment to the Direction of the Jardin des Plantes, Thuret carried on his algological work for a time alone, ultimately associating himself with Dr. Bornet, who is happily still living, and occupied with the gradual publication of their joint and classical work.

In 1858 Decaisne began the publication of "Le Jardin Fruitier du Museum," which was only brought to completion within a few years of his death. This in form is one of the most sumptuous of modern botanical books; in matter it is a monument of patient labour on the cultivated forms of fruit-plants, elaborated in the thorough spirit of the naturalist; its value in a scientific sense will gain with time when the races figured and described in it are supplanted and lost. Students in the future will turn to Decaisne's minute and laborious pages to compare the phases of variation which he has permanently recorded. In much other work of this kind he had the collaboration of his friend Naudin, now director of the botanical station at Thuret's country seat at Antibes, which the heirs of the latter presented to the French Government.

Decaisne did not, however, by any means devote all his energy to the study of the forms of cultivated plants under his charge. He was always occupied with the elaboration of careful dissections and descriptions of new and interesting genera and species of plants. He was an admirable draughtsman, and the "Traité Général de Botanique, Descriptive et Analytique," which he published in conjunction with Le Maout in 1868, is a perfect treasure-house to the student, enriched on every page with the results of his minute and careful observations and the work of his accurate pencil.

Besides this elaborate review of the whole vegetable kingdom, Decaisne published many useful contributions to detailed systematic botany. Among these may be mentioned his classical memoir on the *Lardizabaleæ* (1837), that on *Pomaceæ*, and the *Asclepiadeæ* and *Plantagineæ*, elaborated for De Candolle's "Prodromus."

To the period of his attachment to the herbarium of the Jardin des Plantes belong his few but standard contributions to phytogeographic literature. These include papers on the plants of Arabia Felix, Egypt, Sinai, and Timor. Morphological botany owes to him, besides the memoir on madder already mentioned, a well-known research (1841) on the development of the pollen and ovule and the structure of the stem of mistletoe (*Viscum album*), and a study on the floral organogeny of the pear (1857), the excellence of which shows how much Decaisne could have done in this branch of botany had his circumstances and leisure allowed him to devote more attention to it.

In 1842, on the death of Guillemin, he was associated with Adolphe Brongniart in the editorship of the botanical part of the "Annales des Sciences Naturelles," and on the death of the latter became sole editor. In 1847 he was elected a member of the Institute, taking the place of Dutrochet. In 1864 he was President of the Académie des Sciences; and in 1877 was elected a Foreign Member of the Royal Society. He was one of the principal founders of the Botanical Society of France in 1854, and as long as health allowed was an assiduous attendant at its meetings.

The note of Decaisne's scientific work was patient and laborious observation of facts. He was one of those whose life was spent in making sure the foundations of taxonomic botany, and every subsequent worker will build the more securely for what Decaisne has done. That his mental character was essentially precise and matter-of-fact is merely what we should expect from the direction of his work. From this no doubt it followed that the doctrines of evolution, which in England and Germany have given a new impulse to biological study, interested him little. Not that his mind was wanting in flexibility to new ideas: he warmly supported the investigations made by Bornet in confirmation of Schwendener's theory as to the nature of lichens—a view which has met with much opposition from the older botanists who are, for the most part, unwilling to abandon the conception of lichens as autonomous organisms. His method of work, partly no doubt cramped by the inroads on his time of official duties, made him more and more disinclined to generalise from large series of facts. In striving for minute accuracy he, especially in his later work, somewhat lost the gift of seeing things in a larger perspective.

He was never married, and to his friend Bornet fell the lot of watching his last moments.—W. T. T. D.

SIR JAMES ALDERSON was born at Hull on the 30th December, 1794. He was the youngest son of Dr. John Alderson, an eminent physician of Hull, who had for many years an extensive practice in the East Riding of Yorkshire. Young Alderson received his early education at a school in his native town kept by the Rev. George Lee, a Dissenting clergyman, and afterwards (in the year 1818) proceeded to Cambridge and entered at Pembroke College, and four years after took the degree of B.A. with the distinction of being Sixth Wrangler of that year. He was soon afterwards elected a Fellow of Pembroke, and in due course received the degree of M.A. Having chosen medicine as his profession, he pursued his studies in London and Edinburgh, and subsequently went to Oxford, and being incorporated at Magdalen Hall, received the M.D. degree in 1829. The following year he became a Fellow of the College of Physicians, and for a short time settled in London with the object of practising his profession, during which period of his career he held the office of Physician to the Public Dispensary. On the death of his father, however (in 1829), he removed to Hull, where he rapidly acquired a large consulting practice in the town and neighbourhood. He was much respected by his professional brethren and by the general public, and took a warm interest in promoting every scheme likely to advance the educational movement of the day.

In 1845 Dr. Alderson returned to London, and was elected one of the Physicians of St. Mary's Hospital, the duties of which appointment he discharged with great zeal for nearly twenty years; and on his retirement after this long service, he still retained a connexion with the institution by accepting the compliment paid to him by the Governors in unanimously electing him Consulting Physician.

Having been elected a Fellow of the College of Physicians in 1830, and shown much interest in the changes which were gradually taking place in the medical corporations, and more especially in the College of Physicians, he was appointed Treasurer of the College in 1854, an office for which, from the methodical and exact bent of his mind, he was peculiarly fitted. This office he resigned in 1867 on his being chosen President of the College, receiving the honour of re-election in the four following years. He was the representative of the College at the

General Council of Medical Education and Registration for 1864, 1865, and 1866. In 1869, Dr. Alderson had the honour of knighthood conferred on him, and in the following year the honorary degree of D.C.L. by the University of Oxford.

Five years later Sir James Alderson was appointed Physician Extraordinary to Her Majesty.

Though an observant and experienced physician, he was not a frequent contributor to the literature of his profession. He delivered the Lumleian Lectures in 1852 and 1853, and, what is unusual, was twice appointed to deliver the Harveian Oration, viz., in 1854 and 1867. He pointed out and described collapse of the lung in connexion with whooping-cough in a paper read before the Medico-Chirurgical Society, and published in the Transactions of that Society.

He was also the author of a work, published in 1847, on "Diseases of the Stomach and Alimentary Canal," in which he embodied the result of his extensive experience in a most important class of diseases.

Charles Ansell, a distinguished actuary, for many years known as the father of his profession, was born in December, 1794. When 14 years old, he entered the "Atlas" office as a junior, and two years later was appointed on the staff. In 1823 he was raised to the post of Actuary of the Company, which he retained till 1864, a period of 41 years. He then retired, but retained the office of Consulting Actuary. He held the same responsible post in several other societies, and among them the National Provident, of which he completed the *Bonus* investigation when in his 80th year—a notable *tour de force* for one of so advanced an age.

Mr. Ansell was often consulted by the Government on subjects bearing on national finance, and in 1864 was warmly commended by Mr. Gladstone. He was also examined before several Select and Royal Commissions, on questions involving actuarial considerations. He was, however, best known for the services he rendered to Friendly Societies, on which subject he wrote a valuable treatise in 1835, having been elected F.R.S. the previous year. This treatise was issued as one of the series published by the Society for the Diffusion of Useful Knowledge, which body is credited with the first successful attempt to collect facts bearing on the sickness and mortality of the working classes of England. The Society caused schedules to be printed and circulated, and those returns which were least imperfect among them Mr. Ansell used as materials for his tables and calculations. The comprehensive and scientific character of this work may be inferred from the fact that it treats of "The Doctrine of Interest of Money" and "The Doctrine of Probability," contains numerous tables, and an appendix of the Acts of Parliament relating to friendly societies. A large professional practice followed naturally on this publication. He died December 14, 1881, at the advanced age of 87.

Decimus Burton, born September 30, 1800, was, as his name implies, the tenth son of James Burton, one of the most enterprising and successful builders of his day. He was practically educated as an architect in the office of his father, who at that time was extensively engaged under Mr. Nash's superintendence in designing and erecting the terraces which surround the Regent's Park, and also the Regent Street improvements. In consequence of this employment, he has been credited with the design of several of those terraces, but there seems no foundation for the report, and none of them show that careful study of design and detail, which marked all his subsequent works. After leaving his father's office, Mr. Burton completed his professional education in the office of Mr. George Maddox, at that time an architect in considerable practice. He then, without any interval devoted to foreign travel or other preparation, entered at the early age of twenty-one on the active duties of his profession, and owing apparently to the excellent introduction afforded by his father's connexion, he commenced with an amount of employment which seems never to have failed him during the fifty years that he continued the practice of an architect. One of his earliest works was the villa which he built for Mr. Greenough in the Regent's Park, which as originally erected was one of the most elegant and successful adaptations of the Grecian style to purposes of modern domestic architecture to be found in this or any other country.

In 1823 he erected, on the east side of the Park, the Colosseum to contain the Panorama of London, drawn by Mr. Horner. This, however, can hardly be said to have been a successful design, though it was only another exemplification of the difficulty of combining the rectilineal lines of a classical portico to the circular form of a domical structure. The Roman architects, even with their more flexible style, failed in producing a happy result in the Pantheon, and it is therefore not to be wondered at that we saw the work of a modern architect who attempted the same thing disappear without an expression of regret. He was far more successful in the arches which, in 1825, he was commissioned to erect on Hyde Park Corner.* The arch leading to Buckingham Palace, though somewhat lacking in originality of design, is a singularly elegant adaptation to a perfectly legitimate purpose, of the Roman triumphal archway. The screen opposite leading into Hyde Park, though showing quite as much elegance, is more original, and like some of the park lodges which he erected at the same time, evinces a mastery of the elements of design in the classical styles that has rarely been surpassed.

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* One of the minor sorrows of Mr. Burton's life was the disfigurement of this arch by its being used as a pedestal for the Duke of Wellington's statue, a purpose for which it was singularly ill-suited. He felt this most keenly, but had he lived a year longer, he probably might have been consoled by its removal.

Shortly after this (1827) the Athenæum Club was erected from his designs, and this, considering that it was one of the first of its class, and that a very small sum only was placed at his disposal, may be considered as one of his most successful works. The entrance hall and staircase are not surpassed for dignity of design by anything in any club in London, and the drawing-room, both in its proportions and details, is one of the most beautiful rooms, of its class, anywhere to be seen.

Before these works were completed he received a retainer from Mr. James Ward to lay out and design the villas and buildings of Calverley Park, Tunbridge Wells. This occupied his time almost entirely for the next few years, and after that he seems to have been no longer ambitious of public employment, but to have been content with the practice of his profession among a numerous body of constituents who kept him fully and profitably employed in a far more agreeable manner than in the struggle for what are considered the great prizes of the profession. All his works consequently in which the public feel much interest were erected during the first ten years of his professional career. During the remaining forty, till his retirement in 1869, he erected and altered innumerable houses, and especially horticultural edifices for the Dukes of Devonshire and Northumberland, and other noblemen and gentlemen, by all of whom he was treated more as a friend than as a professional adviser. The extreme amiability of his character, and his thoroughly gentlemanly conduct in all his business arrangements, endeared him to all with whom he came in contact. He was elected a Fellow of the Royal Society in 1832.

In no instance was Mr. Burton ever suspected of sacrificing the interest of a client for his own glorification, or for the indulgence of his own individual fancies. Having placed at his client's disposal all the resources of his long experience, and sound practical sense, and good taste, he set at once loyally to carry out the wishes of his employer with an amount of self-negation rare in the profession. Few men have consequently gone through a long professional career with a more numerous body of friends and fewer enemies.

Though declining to compete for any of the great works at the disposal of Government, Mr. Burton never ceased to be connected with Her Majesty's Board of Works. All the buildings in Kew Gardens were erected from his designs and under his superintendence. So were those in the Phœnix Park, Dublin; and the Embassy in Paris was for some time under his charge. All the buildings in the Royal Botanic Gardens and the Zoological Gardens were designed by him. Curiously enough he never during his long career was called upon to erect any church or ecclesiastical edifice of any importance, but he was employed conjointly with his friend Sidney Smirke in the restoration of the Temple Church.

In 1869 Mr. Burton retired from the active pursuit of his professional duties, having realised an ample competence by their exercise, and spent the remaining years of his life partly at St. Leonard's, where he had built himself a charming villa residence, and partly in London, in Gloucester Houses, where he died. Notwithstanding his failing health during these last few years, he continued to dispense the most genial hospitality, and to enjoy the social intercourse of his numerous friends to the very last He died the most peaceful of deaths at the ripe old age of eighty-one years, December 14, 1881.

J. F.

By the death of the Rt. Hon. Sir James Colvile, the Society lost a Fellow of the Privileged Class, one whose disinterested and judicious labours as President of the Asiatic Society of Bengal, contributed materially to the progress of science in British India for a decade of years, during which time all the resources of his cultivated intellect, of his high official position, and of his hospitable house, were as unobtrusively as liberally placed at the services of the Society, and its members individually.

James William Colvile, born in 1810, was the eldest son of Andrew Wedderburn Colvile, Esq., of Ochiltree and Crombie in Fife, and the Hon. Louisa Mary Eden, sister of the second Lord Auckland; the latter as Governor-General of India, and subsequently as First Lord of the Admiralty, was no less distinguished than his nephew for his efforts in the advancement of science. From Eton he went to Trinity College, Cambridge, graduating as M.A. in 1834, after having attained the rank of Senior Optime; the late Bishop Selwyn being Junior Optime in the same year. At Cambridge he formed what proved to be a life-long friendship with our Fellow, Lord Houghton. He was called to the Bar (Inner Temple) in 1835, and practised as an equity draftsman for ten years at chambers in Lincoln's Inn. In 1845 he accepted the office of Advocate-General to the Hon. East India Company; proceeding thereupon to Calcutta. In 1848 he was raised to the Bench as Puisne Judge of the Supreme Court of Bengal and, as is usual in such cases, was knighted. In 1855 he succeeded to the Chief Justiceship, which he held for four years, retiring and returning to England in 1859.

During his residence in India Sir James married Frances Elinor, daughter of Sir John Peter Grant, K.C.B., G.C.M.G., of Rothiemurchus, formerly Lieut.-Governor of Bengal, and subsequently Governor of the Island of Jamaica. Lady Colvile survives him, but their only child, a son, died in early youth.

Immediately after his return to England Sir James was sworn in as a Privy Councillor, and made one of the assessors to the Judicial Committee of the Council on Indian Appeals; his fellow assessor being Sir Lawrence Peel, his predecessor in the Chief Justiceship of Bengal, and his most intimate friend. In 1865 he became a member of the Judicial Committee itself, and in 1871 (on the re-organization of the Council) he was chosen one of the paid judges of the same.

Sir James Colvile's legal attainments were of a very high order. It is written of him by a competent judge, "His knowledge of Indian systems of law, and his acquaintance with India were highly valued by his colleagues and by suitors; and his judgments were full and exhaustive statements, often of cases intricate and involved in the highest degree. According to the custom of the Privy Council, they embodied the opinions, which he had assisted to form, of other judges; the practice of a separate judgment being delivered by each judge not having taken root."

It was, however, as the wise, the calm, the considerate President of the Asiatic Society of Bengal, that Sir J. Colvile's name and memory are esteemed by every scientific man who had the privilege of knowing him in India. He occupied the chair from the date of the resignation of his genial predecessor, Governor-General Lord Hardinge, in December, 1847, till that of his own departure from England in 1848. During not a few years of that interval, when the Society's affairs were troubled, and itself not exempt from internal dissensions, it was steered through its difficulties by the inexhaustible patience, sound judgment, firmness, and conciliatory measures and manners of its watchful chief. It need hardly be added, that when he left India, the Council of the Society placed on record its feelings of regret at the loss of his valuable services, and its thanks for the zeal and ability with which he had for ten years discharged the office of President, and had promoted the objects and interests of the Society.

After his return, Sir James resided during the autumn vacation at his seat, Craigflower in Fife, on the banks of the Forth, a few miles west of Dunfermline, a property which he inherited and to which he was devotedly attached. He died at his residence in London, 8, Rutland Gate, from a sudden cessation of the heart's action, preceded, however, by a gradual failure of both the digestive and circulatory powers. He was elected a Fellow of this Society on April 29th, 1875, being one of the first under the modified rules for the election of persons of the Privileged Class, which restricted these to members of the Privy Council.

J. D. H.

Dr. George Dickie was born in Aberdeen on the 23rd November, 1813, and received his education there, graduating M.A. in Marischal College, in 1830. He thereafter commenced the study of medicine in his native city, completing his medical education in Edinburgh, where he gained the Medal for Pathology and Practice of Medicine in the Brown Square Medical School, in 1833. In 1834 he became M.R.C.S. of London, and in 1842 received the honorary degree of M.D. from King's College, the University of old Aberdeen.

He originally intended to enter the naval medical service, but abandoned that intention, and entered on medical practice in Aberdeen. His tastes, however, lay very strongly in the direction of natural science, especially botany, and in 1839 he was appointed Lecturer on Botany in King's College; and subsequently on Materia Medica and on Zoology, and he further held the office of Librarian to the University. In 1849 he resigned these offices, having been appointed to the newly created professorship of natural history in the University of Belfast; but in 1860, on the establishment of a professorship of botany in that of Aberdeen, he was appointed to it and returned to his native city. Soon after his return he suffered from a severe attack of illness, which resulted in increasing deafness and more or less chronic bronchitis; but he continued to discharge the duties of the professorship till 1877, when the state of his health obliged him to resign.

In 1838 he became a member of the Edinburgh Botanical Society (of which he was elected a Honorary Fellow in 1877), in 1863 he was elected F.L.S., and in 1881 F.R.S. He was also a member of the Société des Sciences Naturelles de Cherbourg, and of several local Societies.

Dr. Dickie began in early life to investigate the flora of the district around Aberdeen, the results of which he published in 1837; and from that time onwards he published numerous articles chiefly on the morphology and physiology of plants in the Journals and Transactions of scientific Societies. In 1844 appeared his first article on Algæ, to which group he devoted more and more of his time, and to which his published articles of late years almost exclusively relate. He also contributed botanical appendices to the works of various Arctic travellers and voyagers and to the reports on the Transit of Venus expeditions, and he worked out the Algæ of the "Challenger" expedition. The value and care of his investigations as an observer are attested, and the assistance rendered by him acknowledged, in Harvey's "Phycologia Britannica," Ralf's "British Desmideæ," Smith's "British Diatomaceæ," and Macgillivray's "Natural History of Deeside and Braemar." He was author of "A Flora of Aberdeen" (1838), "The Botanist's Guide to the Counties of Aberdeen, Banff, and Kincardine" (1860), and "A Flora of Ulster" (1864). In all these works he enumerates the vascular plants of the districts treated of, and gives much information as to their localities and altitudinal range, and in the "Botanist's Guide," he includes the cellular cryptogams so far as known to himself, and of the marine Algæ in particular he gives a very complete list.

Dr. Dickie did not confine his studies to botany, but wrote also on zoology, treating chiefly of morphology. In conjunction with Dr. McCosh he was author of "Typical Forms and Special Ends in Creation," to which he contributed those parts that relate to botany, zoology, physiology, geology, and physical geography. In this work the authors seek to indicate the evidences of design in the universe discoverable alike in the general principles that prevail throughout, and in the special adaptations of organised beings to their environments.

In private life Dr. Dickie was much esteemed. He was ever ready to aid those in need of assistance in every way in his power; and as professor gained the respect and esteem of his students, not a few of whom owe their success in after life to his devotion to their culture and future careers.

Dr. Dickie's constitution was never robust; he overtaxed his strength when young, and in middle life by his exertions in the field as an observer and collector, and he suffered from chronic deafness. During an excursion in Braemar with his students, in 1861, he exposed himself for several days of very severe weather, which resulted in bronchitis and its complications, from which he never entirely recovered. This and increasing deafness latterly cut him off from personal communication with strangers, but his correspondence and his interest in scientific pursuits were never relaxed. In the early part of this year he had a severe attack of bronchitis, which after recoveries and relapses carried him off on the 15th July, 1882.

Dr. Dickie married, in 1856, Miss Agnes Low, of Aberdeen, who survives him, and by whom he had six children.

Ronald Campbell Gunn was an ardent naturalist, and to his exertions far more than to those of any others we are indebted for a knowledge of the botany and zoology of his adopted colony, Tasmania.

Mr. Gunn, the son of an officer in the army, was born at Cape Town on April 4th, 1808, and was brought thence to Bourbon, at the capture of which place his father assisted. Until 1816 he accompanied his father's regiment to the Mauritius, the West Indies, and Scotland, where he resided for eight years. His name was placed on the Commander-in-Chief's list for a commission in the army; this, however, he never received: but an appointment in the department of the Royal Engineers at Barbadoes was given him, which he held till 1829, when he emigrated to Tasmania. Here he became successively assistant superintendent of convict barracks at Hobarton, superintendent of convicts for the northern division of the island, police magistrate, and coroner, first at Circular Head and lastly at Hobarton.

It was not till 1831 that Mr. Gunn's taste for science was developed, and this was due to the example of a young colonial naturalist of high promise, Mr. William Lawrence, who died within a year after having inoculated his friend with a passion for his own pursuits. Up to this time the natural history of Tasmania, an island as large as Ireland, was known only through the partially published botanical collections of Robert Brown, made at two spots only during Flinders' memorable voyage at the beginning of the century; with this exception it was both botanically and geologically almost a *terra incognita.* Provided with introductory letters by Mr. Lawrence to Sir W. Hooker and Dr. Lindley, Mr. Gunn entered into an active correspondence with those gentlemen, which he maintained with unflagging zeal and interest for upwards of a quarter of a century, travelling all over the island, transmitting to England carefully preserved specimens accompanied with copious notes and descriptions, and receiving in return books, instruments, and appliances whereby he rapidly acquired a good scientific knowledge, especially of the botany of the island, which greatly enhanced the value of his successive collections.

Mr. Gunn's labours were not confined to botany; through the above-named friends he was introduced to Dr. J. E. Gray and other officers of the British Museum, to which and to other bodies he contributed magnificent collections of mammals, birds, reptiles and mollusca. He further made himself so competent a geologist that he was employed by the Government to report on various public works and on the goldfields, and especially on alleged discoveries of the precious metals. Of his general ability and the confidence reposed in him no higher testimony could be conceived than the fact of his having been in 1864 appointed at the request of the Government of New Zealand, one of three commissioners (the others representing New South Wales and Victoria) for determining the most suitable site for the capital of New Zealand, a quarter of a century after its colonisation; their inquiries resulted in the unanimous selection of Wellington.

A man of the energy, ability, and attainments of Mr. Gunn was not likely to escape the notice of such a Governor of the Colony as Sir John Franklin who, almost immediately after his arrival, appointed him to the office of Clerk to the Executive and Legislative Councils, to which Sir John added that of private secretary to himself. Government House became at once a small scientific circle, and a close intimacy sprang up between Mr. Gunn and

Sir John and Lady Franklin, which lasted through their lives. This coterie was soon largely though only temporarily augmented by the arrival in Hobarton in 1840 of the Antarctic Expedition under Sir James Clark Ross, who together with some of his officers, assisted Sir John and Lady Franklin and Mr. Gunn in founding the "Tasmanian Journal of Natural Science," the reading of papers for which took place in the drawing-room of Government House. From this small beginning sprang the Royal Society of Tasmania, and the nascent periodical subsequently grew into the "Proceedings of the Royal Society of Van Diemen's Land," of which Mr. Gunn was the editor, as he had been of the Journal, from the first.

Unfortunately Mr. Gunn's health broke down under the close confinement and long hours of office work at Hobarton; and after fulfilling various duties in Launceston and elsewhere, including that of member of the Legislative Council for Launceston, and of the House of Assembly for Selby, he was compelled to retire from the public service in 1876. He eventually succumbed, March 12, 1881, to attacks of creeping paralysis complicated with disease of the lungs.

Mr. Gunn's published labours are few, but the results of his collections and copious observations are embodied in various works on Australian science, and especially in Sir J. D. Hooker's "Flora of Tasmania," and in Mr. Gould's "Birds of Australia." In conjunction with the late Dr. J. E. Gray, he published notes and descriptions of the mammals and fish of Van Diemen's Land, and he was the author of a few other papers on the geology and some on the botany of that island, together with one on the encroachments of the sea on the north coast of Tasmania; he further contributed to "West's History of Tasmania" a compendium of its zoology.

Mr. Gunn was elected a Fellow of the Linnean Society in January, 1850, and of the Royal Society on June 1, 1854. J. D. H.

JOHN SCOTT RUSSELL, the eldest son of the Rev. David Russell, of Clydesdale, was born in 1808, and displayed at an early age a great predilection for mechanics and natural science. After some preliminary practical training he studied at the Universities of Edinburgh, St. Andrew's, and Glasgow, at which latter he graduated at the early age of sixteen. On the death of Sir John Leslie, Professor of Natural Philosophy in Edinburgh in 1832, Mr. Russell, being then only twenty-four years of age, was appointed temporarily to carry on the work of the chair during the session 1832–33.

About this time he commenced his well-known researches on the nature of waves, and the resistance of fluids to the motion of floating bodies. His first paper on this subject was read before the British Association in 1835, his deductions being founded on a very large and elaborate series of experiments. In 1837 he read a second paper on the same subject, before the Royal Society of Edinburgh. The Society conferred on him their Keith Prize (a gold medal and a sum of money) in 1838, and elected him into their Council.

His principles of ship construction were first carried into execution in a vessel called the "Wave," built in 1835, and, subsequently, in many others, including the then new fleet of the West India Royal Mail Company, built by a company at Greenock, of which Mr. Russell was the manager.

He was elected in 1847 a member of the Institution of Civil Engineers, where he served on the Council, and was chosen one of the Vice-Presidents. In 1849 he was elected a Fellow of the Royal Society.

Mr. Scott Russell removed to London in 1844, and became well-known as a ship-builder on the Thames. In the capacity of contractor, and also assisting by his advice, he built the Great Eastern steamship, under the direction of the late Mr. Brunel, and constructed the paddle-wheel engines of that large ship.

His last work in naval construction was the steamer on the Lake of Constance, which carries railway trains between the termini of the German and Swiss railways.

Besides naval construction, Mr. Russell practised in other fields of engineering, one of his principal works being the great dome of the Vienna Exhibition of 1873.

Mr. Russell was appointed joint secretary with Sir Stafford Northcote of the Exhibition of 1851.

Although not a contributor to the papers in the Royal Society, yet he was the author of more than forty papers read at other Societies, including the Royal Society of Edinburgh, the British Association, and the Institutions of Civil Engineers and Naval Architects. In the latter Society he always took an active interest and was one of its earliest promoters.

Mr. Scott Russell was the author of the article on the steam-engine in the Encyclopædia Britannica, also a large work called the "Modern System of Naval Architecture for Commerce and War," and of a work on "Technical Education for the English People."

He was an accomplished linguist, and as a speaker possessed great clearness and skill in exposition. W. H. B.

The subject of this memoir, COLONEL J. T. SMITH, R.E., the son of George Smith, Esq., of Edwalton, Notts, and afterwards of Foelallt, Cardiganshire, was born in or about the year 1805.

Destined for a military career, he was educated at Addiscombe, and in 1825 proceeded to India as an Officer of Engineers.

Upon being appointed to arrange a system of lights for the South

Indian Coast, he entered upon a series of optical investigations, and devised a reciprocating light, which was fixed in the lighthouse erected at Madras in 1838, after his designs.

Before this he had translated Vicat on "Cements," enriching that valuable work with the results of many original experiments, and he was elected a Fellow of the Royal Society in 1837.

He was next appointed to reorganise the Madras Mint, which he did with great skill and vigour. When remodelling the machinery he invented a machine for automatically weighing and assorting blanks, which gained an award at the Exhibition of 1851.

After a period of some twenty years in charge of the Madras, and latterly of the Calcutta, Mint, Colonel Smith returned to England, and, after acting for some time as Consulting Engineer to various Indian Irrigation Companies, became Chairman of the Madras Railway Company, a position which he held until the close of his life.

During late years his energies were directed chiefly to the consideration of the intricate questions of political economy connected with currency, &c. A careful study of the subject for many years, and a deep interest in India, led him to propose a remedy for the evils caused by the depreciation of silver in that country.

He brought forward his views with great earnestness and disinterestedness, and though the question has been forced aside by more pressing matters, his proposals have met with the approval of some of the leading political economists of the day, and it is hoped that by their fulfilment at some future time many millions may be saved to the revenues of India.

His knowledge of these and of kindred subjects led to his employment by the Government to make reports in conjunction with the late Professor Graham, F.R.S., upon questions of mintage, &c., and he was also appointed to attend the International Monetary Conference held at Paris in 1865.

Colonel Smith's active mind and varied talents led him to take an interest in many other subjects. He was a member of several societies, and the author of some works and several papers on various scientific matters.

In private life he was an humble Christian man. His aid as a wise and sympathising counsellor was widely sought, and his loss is deeply felt by a large number of friends.

P. S.

In William Newmarch the city of London lost its most distinguished man of business, and the scientific world one of that small group of diligent and learned men who have most successfully brought the universal methods of logic and science to bear on the social life of man.

Mr. Newmarch was a Yorkshireman, born at Thirsk, January 28, 1820. He took his schooling at York, and, as a young man, held some clerkly appointments in that city. He early showed his literary taste and talent by publishing a Guide to the City, by frequent correspondence with the "Sheffield Iris," and by the delivery of lectures. Having served as a clerk under a stamp distributor, he passed to the Yorkshire Fire and Life Office at York, then to the banking house of Messrs. Leatham, Few, and Co., at Wakefield. From Wakefield he moved to London, and served on the staff of the Agra Bank. In 1852, he was appointed Secretary of the Globe Insurance Company, and took the lead in conducting the negotiations which resulted in the amalgamation of that office with the Liverpool and London Insurance Company. In 1862 he accepted the post of Manager in the banking house of Messrs. Glyn, Mills, Currie, and Co., which he did not resign till he had warnings of an attack of paralysis.

Mr. Newmarch always found the performance of his duties as an official consistent with an extraordinary amount of literary activity, coupled with earnest work on behalf of the many societies and public bodies to which he attached himself, and of some of which he was the founder. Among these we may mention the Cobden, Adam Smith, and Political Economy clubs, the Institutes of Actuaries and Bankers, and the Statistical Society. He was on the staff of the "Morning Chronicle," an occasional contributor to the "Times" and other leading papers, and a constant correspondent of the "Economist." His writings, for the most part anonymous, were devoted to questions of currency, banking, free trade, and the laws of industrial and commercial progress. In the Statistical Society, to which he was always warmly attached, he held in succession the offices of Honorary Secretary and Editor of its Journal, President for the usual term of two years (in succession to Mr. Gladstone), and Honorary Vice-President. As Editor of the Journal of the Society, he planned several of its periodical returns and tables, and he contributed to its pages many important papers and presidential addresses. To the last he continued to take a lively interest in the question of providing suitable house accommodation for the Statistical and cognate Societies; and made a strenuous but unsuccessful effort, by means of a company with limited liability, to carry this cherished object into effect. In 1861, he presided over the section of Economical Science and Statistics at the Session of the British Association held at Manchester.

One of Mr. Newmarch's series of papers, that on the New Gold Supplies, contributed to the pages of the "Morning Chronicle" in 1853, was afterwards printed as a separate volume. But the work on which his claim to permanent distinction must rest is that in which he took part with Mr. Thomas Tooke (also a Fellow of this Society), namely, the "History of Prices" published in 1856, but now out of print. The two concluding volumes were from the pen of Mr. Newmarch. This work was at once recognised in England as a complete and masterly exposition of the great economic questions of the second quarter, and early part of the third quarter, of this century; namely, the Introduction of Free Trade, the Bank Charter Act, with the Development of the Banking System at home and abroad. The Gold Discoveries in Russia, California, and Australia, the Irish Famine and Emigration, and the Crimean War. Mr. Newmarch is understood to have been bent on continuing this work up to the present date; but, unfinished as it was, it was speedily accepted as a classic, and translated into German. It was to this work that he was chiefly indebted for his election as Corresponding Member of the Institute of France. He was chosen F.R.S. in 1861.

On the death of Mr. Tooke, Mr. Newmarch showed the high estimation in which he held him by taking the leading part in founding "The Tooke Professorship of Economic Science and Statistics" at King's College.

Mr. Newmarch was examined by the Select Committee on the Bank Act, and gave evidence before several Parliamentary Committees on economic questions, such as the currency and the income tax.

Mr. Newmarch was a fluent and effective speaker, and his writings are all marked by clearness, directness, and vigour. He held his own opinions with so firm a grasp that he was rather intolerant of opposition, and in debate appeared perhaps somewhat wanting in courtesy; but those who knew him well found in him a generous and warm-hearted man, an encourager of rising talent, and a firm friend. Take him all in all, he was assuredly one of the most remarkable among the self-made men of this century. He died at Torquay, March 23, 1882, after an illness of some duration, which began as an attack of palsy in 1881, from which he partially recovered. He had lost his only son through a painful illness, leaving the son's widow and his own, with one daughter, to lament his loss. W. A. G.

PROCEEDINGS

OF

THE ROYAL SOCIETY.

April 20, 1882.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The President condoled with the Meeting on the great loss which Science and the Society had sustained through the decease of their distinguished Fellow, Charles Darwin; and mentioned that a proposal for interment in Westminster Abbey had been made to the family of the deceased and to the Dean, which he trusted would be brought to pass.

The following Papers were read:—

I. "On the Formation of Ripplemark." By ARTHUR ROOPE HUNT, M.A., F.G.S. Communicated by Lord RAYLEIGH, F.R.S. Received March 28, 1882. Read April 20.

Fossil ripplemarks are often appealed to by geologists as evidence that the winds and waves that formed them cannot have differed much in intensity from those that produce similar corrugations on the sands of our modern beaches; but, although considerable value is attached to the evidence afforded by these relics of ancient seas, authorities differ much as to their origin.

According to Sir Charles Lyell ripplemark originates in the "drifting of materials along the bottom of the water," and "is usually an indication of a sea beach, or of water from 6 to 10 feet in depth, though this rule he proceeds to say is not without exception, as recent ripplemarks have been observed at the depth of 60 or 70 feet. The crossing of two sets of ripples the distinguished author ascribes to the new direction in which the waves are thrown on the shore consequent on a change of wind ("Elements of Geology," 6th edition, p. 19). Mr. J. Beete Jukes, in the first edition of his "Manual of Geology," states, that current mark or ripple "is produced on the sea beach, not in consequence of the ripple of the wave impressing its own form on the sand below, which would be an impossibility, but because the moving current of water as the tide advances or recedes produces on the surface of the sand below the same form as the moving current of air produces on the surface of the water above. A rippled surface, therefore, to a rock is no proof of its having been necessarily formed in shallow water, though rippled surfaces are perhaps more frequently formed there, but simply a proof of a current in the water sufficient to move the sand at its bottom gently along, at whatever depth that bottom may be from the surface of the water." Speaking of fossil ripplemarks the same author states that the distance from crest to crest of the ridges varies from half an inch to eight or ten inches, with a proportionate variation in depth between them (Jukes' "Man. Geol.," p. 172). The article on ripplemark is recast in the third edition, edited by Dr. Geikie and published in 1872, but the views expressed therein are the same.

Monsieur Delesse goes far beyond the authorities referred to above, as on the authority of Commandant Cialdi, he states that the movement of waves can displace fine sand at a depth of 200 metres in the ocean, and (without giving his authority for the statement) that the undulation of the sea is evidenced by ripplemarks on muddy bottoms down to a depth of 188 metres ("Lithologie du fonds des Mers," 1871, pp. 110, 111). More recently, Mr. G. H. Darwin has stated that one of the conditions of the formation of many ripples is a great ebb and flow of the tides ("Nature," vol. xxv, p. 214).

It will be seen from the authorities cited above, that the phenomenon known as ripplemark is variously ascribed to the action of currents, and to the undulation of waves, and that whereas by some it is considered the result of tidal action in shallow water, by others it is attributed to the action of waves down to the great depth of upwards of 100 fathoms.

I shall endeavour in the present paper to prove that ripplemarks formed under water are, as a rule, completely independent of the rise and fall of tides, of tidal currents, and of sea beaches; and that they have little in common with the current mark, that owes its origin either to a continuous current of air or of water.

For some years past I have neglected no opportunity of making observations on the action of storm waves on the bottom of Torbay, and of collecting evidence as to the action of waves and currents on the bottom of the English Channel. To Lord Rayleigh I must express my indebtedness for having examined my evidence of submarine wave action from a mathematical standpoint, and for having called my attention to the fact that waves, if they affect the bottom at all, do so by setting up alternate currents:* and that, though at great depths the action is very small, theoretically it has no limit.

Having observed that ripplemarks are commonly better preserved in pools between tidemarks than on those parts of the tidal strand left dry at low tide, and that the bottoms of these pools must be in some measure protected from the continuous currents that are commonly supposed to produce the ripplemarks, it seemed to me probable that they were produced by the alternate action of waves described to me by Lord Rayleigh.

One fine and almost calm day in the summer of 1881, being at Broadsands in Torbay, and seeing that the strand was covered with ripplemarks, I proceeded to watch carefully the action of the water with a view of ascertaining, if possible, the process of their formation. Floating in my boat a few yards from the shore in about 18 inches of water, I narrowly scanned the effect of the very gentle swell that was breaking on the beach. I observed that a small shell lying in one of the furrows instead of being steadily washed shorewards by the incoming waves, was washed backwards and forwards from one furrow to another; sometimes it would stop on the intervening ridge, and so for the moment help to build it up; at others it would fall over into the furrow towards which for the moment it was being propelled, but in no case did it show any tendency to travel continuously in any particular direction along the bottom.

On a subsequent occasion, having to land on the beach at Paignton, and seeing the ripplemarks well developed, I again carefully watched them seawards. At a point where the bottom was too indistinct for me to observe its condition, I could distinguish fragments of seaweed gently moving backwards and forwards in the direction of the beach, and at right angles to the ripplemark where last visible. This observation was unexpected, as it proved a gentle swinging motion of the water in the vicinity of the shore, when the surface motion was so slight as not to interfere with my landing on a flat open beach from a very small boat.

On the 19th October, 1881, there was a strong south-easterly gale in Torbay, and the waves rolled on to the Meadfoot Sands at the rate of 7½ per minute. The distance between the southern point of an outlying islet known as the Shag Rock .and the rocks at the western end of Meadfoot Sands (two points in line with the direction of propagation of the waves), 275 yards by the chart, was covered by exactly three waves, so that each one must have measured 275 feet from crest to crest. In midbay this length was probably exceeded. As from the manifest turbidity of the water, the bottom was unquestionably much disturbed, I looked forward with interest to ascertaining by the dredge what effect these waves of known dimensions had had on the bottom. On the 31st of October, nearly a fortnight after the gale, I had an opportunity of going out for this purpose, and in places, in 6 fathoms (at low water spring tides), where the bottom is usually a soft muddy sand that clogs the dredge in a few minutes, the ground proved to be quite hard. One haul of the dredge brought up a Buccinum shell, with the mollusc inside it dead, and two dead ascidians; and another in midbay, though with 30 fathoms of rope, produced not a shell or a particle of the usual muddy sand, but only a few red seaweeds that must have come from a distance. Never before in my experience had I found the ground so hard in midbay, nor dredged dead molluscs and ascidians. On the 11th November the ground was still very hard, both the dredge and a fishing-lead tied to a line bumping along as though over ridges. On the 8th December, more than six weeks after the gale, I again tried the same spot in midbay that proved so hard on the 31st October: it had now returned to its normal state, and the dredge brought up the usual muddy sand. These dredgings tended to show that the bottom had been violently agitated by the storm, and that as the seas subsided it had become strongly ripple marked. Why it should change from soft to hard and back again to soft is not very clear, but there is no doubt as to the fact.

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* "Trans. Dev. Assoc.," vol. x, p. 191 (1878).

It may be objected that as fossil ripplemarks have been said to be limited to 10 inches, a dredge would scarcely detect modern ripples if not larger than that; but there is no doubt that modern ripplemarks occasionally far exceed these dimensions. I have myself seen them formed in Brighouse Bay, on the coast of Kirkcudbright, fully 2½ feet from crest to crest, and deep in proportion.*

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* Since writing the above my attention has been directed to the following important, though quite incidental, descriptions of wave-marks on the Goodwin Sands by the Rev. John Gilmore, in his book intituled "Storm Warriors, or Lifeboat Work on the Goodwin Sands." They are as follows:—

"On the Goodwins where the force of the sea is in every way multiplied and the waves break and the tide rushes with tenfold power, the little sand-ripples of the smoother shore become ridges of two or three feet high. It is on these ridges that the lifeboat so continually grounds. As the tide rises she is swept from one to the other by the long sweeping waves; she is swung round and round in the swirl of the cross seas and rapid tide, thumping and jerking heavily each time that she strands."—*Op. cit*, p. 109.

". . The heavy seas have driven the sands into high ridges, and the gullies between these are waist-deep and full of running water with the sand soft and quick at the bottom; through these deep gullies the men have to wade."—*Op. cit.*, p. 215.

". . At last all are on board, but they cannot yet leave the sands, they must wait until the water is high enough to float the lifeboat over the ridge which surrounds her."—*Op. cit.*, p. 222.

Feeling satisfied that the bottom of Torbay in about 6 fathoms at low water spring tides was rippled by the swells following the October gales, I proceeded to construct a small tank, about 9 feet by 3 feet by 1 foot, in order to prove experimentally whether subaqueous ripplemarks could be formed at will, and to what extent their dimensions could be controlled.

Working on this small scale I experienced no difficulty in forming good ripplemarks varying in size from $\frac{1}{8}$th of an inch to 4 inches from crest to crest. The tank was commonly arranged as follows:—The sand was so piled up at one end that the waves when generated would quickly tear down what they wanted for a strand on which to break, and from that strand outwards the amount of sand used was regulated by the depth of water required for each experiment. The further end of the tank where the waves were generated was kept free from sand so as to have the greatest available depth of water, generally about 9 inches. The waves were generated by a vertical displacement of the water, either by means of a V-shaped trough worked by hand or by means of a semi-cylindrical block of wood worked by a small model steam-engine.

The following five experiments will show how very rapidly ripplemarks can be formed.

(1.) Waves 60 per minute, height trough to crest about $1\frac{1}{2}$ inches. Result, $1\frac{1}{2}$ inch ripples in water 2 and 3 inches in depth.

(2.) Waves 115 per minute, height not measured. Result, $\frac{1}{2}$ inch ripples well developed in 2 inch water, and discernible down to $3\frac{1}{2}$ inches deep.

(3.) Waves 23 per minute, height not measured. Result, the small ripplemarks now effaced and replaced by others $1\frac{1}{2}$ inch in size.

In the above cases the experiments lasted exactly one minute each.

(4.) Agitated the water at the centre of the tank, gradually getting up an even swing of 13 to the minute. The time was taken after the water was in full swing, and the experiment may have lasted one minute and a half. Result, ripplemarks were now more or less developed over the whole bottom, the largest being 3 inches in length.

(5.) The beach was now removed and the sand levelled over the whole tank. The water was disturbed with an even swing as much as possible. It rebounded from end to end, and dashed over the two ends, which in this experiment were 5 inches above the water-line. The sand being completely stirred up was left a night to settle, and the next day the water being still turbid, it was drawn off. Result, the bottom proved to be strongly but unevenly rippled all over with ripples varying in size from less than 1 inch to over 4 inches; the greatest depression being about $\frac{1}{2}$ inch from trough to crest. In one case a set of ripples had been formed exactly at right angles to a larger set which was nearly obliterated by them.

In a paper published in 1859,* Mr. H. C. Sorby, F.R.S., showed how currents flowing in one direction form the kind of ripplemark or current mark termed by him "rippledrift;" but, as the currents that form the ripplemarks on the sea-shore are alternate and set up no drifting action in the ordinary sense of the word, it seems to me important to distinguish between the current mark that can be seen occasionally on the bottom of running water and the marine ripplemark that differs from it, both as to its origin and as to its effect. I believe the symmetrical ripplemark of the sea-shore cannot be formed by a continuous current, and that whether recent or fossil it is as certain an indication of an alternate wave current as the "rippledrift" is of a continuous current. Both of these current marks can be readily formed in a round tub of water with a little sand on the bottom. If the water be rotated constant-current ripples or "rippledrift" are formed: if the tub be carefully rocked symmetrical alternate-current ripples shortly appear.

My experiments having satisfied my mind that ripplemark can be formed on sandy bottoms by a slight oscillation of the water, I took an early opportunity of visiting the shores of Torbay, between Torquay and Livermead Point, for the purpose of ascertaining definitely whether the size and direction of natural ripplemarks bore any relation to the force and direction of the wind. The day selected was the 21st January, 1882, after a week of calm weather, accompanied by the highest recorded rise of the barometer in Britain. There had been very little wind for days, but a slight swell on the 20th, and very low tides promised a well-rippled beach for the 21st.

On reaching the sands under Sulyarde Terrace, I observed that they were covered with the most perfect and symmetrical ripplemarks from the south-west, the only direction from which a swell from the sea could reach them, as the new pier protects that part of the shore from waves coming from any point more to the southward. Proceeding thence along the sands in a westerly direction, I saw the ripplemarks gradually getting effaced, until at a point opposite the Belgrave Road they were completely obliterated, excepting in pools and depressions in the sand, where they were as perfect as before. At this point, which is not protected by the pier and is exposed to the open sea, the direction of the ripples was south-south-east (S.S.E.). In one of the pools they measured 6 inches from ridge to ridge, and the ridges were sharply defined and perfectly angular. Under the Corbons, on a little beach between the rocks, there were some very perfect ripples 13 inches between ridges, and $1\frac{3}{4}$ inches in vertical height. Passing on to the next beach, Livermead Sands, I found a large area of sand covered by perfect 6-inch ripples from the south-east, which in their turn were crossed by 2-inch ripples from the north-east. The explanation of these cross ripples was clear. A portion of the sands was raised to such an extent that on the tide retiring an island was formed for a short time. The 6-inch ripples owed their origin to the swell from the south-east, whilst all the sands were covered, whereas the 2-inch ripples owed theirs to the water running in and out at the back of the emerged sandbank at right angles to the direction of the main swell.

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* "On the Structures produced by the Currents present during the Deposition of Stratified Rocks." "The Geologist," 1859, p. 137.

On the 23rd January I went over the same ground again, with the following results. Under Sulyarde Terrace I again found the ripples coming from the south-west. Under the Belgrave Road their direction was due south, well developed on flats, but obliterated on slopes towards the sea. In the "submerged forest" clay was a round pool, 15 feet in diameter, with its bottom covered with 5-inch ripples from the south-by-east (S.b.E.), crossed by $1\frac{1}{2}$-inch ripples from east-by-north (E.b.N.) This was owing to the southern side of the pool drying before the eastern. On a small beach between the rocks, at the western ends of the sands, under the Great Western Hotel, the ripples, of different sizes but averaging about 3 inches, came as nearly as possible from the south-east. Under the Corbons Head I again found the large ripples, the largest being 14 inches between ridges and 2 inches high; they were composed of sand, coarser than at Torre Abbey, and broken shells. At the east end of Livermead Sands there was again an extensive low bank, with pools on the landward side. On the bank were 3-inch ripples from the S.E., gradually obliterated towards low-water mark, where the sand was quite smooth. One of the back pools was covered with perfect ripples, varying in size from 2 inches (by estimation, as they were inaccessible) to 17 inches by measurement. Direction of all, south-east. The landward slope of the sand-bank was covered with ripples from the south-east, crossed at different places by others from the north-eastward and eastward. At the west end of the Livermead Sands were some large, but not perfectly preserved, ripples, 22 inches long between ridges and over 3 inches high.

These observations prove that ripplemarks are independent of the direct action of wind, for on two separate occasions the Torre Abbey and Livermead Sands could furnish at the same time ripples coming from all points, from south-west to north-east (on the eastern side). They also show what a complicated problem is that of the size of ripplemarks, and how little the geologist can gather from mere size, for on the same beach were ripples ranging from $1\frac{1}{2}$ inches to 22 inches, and in one small pool almost every size was represented between 2 inches and 17 inches.

Having shown that the conditions requisite for the formation of ripplemarks are alternating currents on a mobile bottom, I will proceed to show that there is good evidence that alternating currents and therefore ripplemarks, occur at much greater depths than is commonly supposed. To do this I must prove that there is occasionally motion at the bottom of the sea, and that this motion does not arise from continuous, but from alternate, currents.

I will commence with a simple case, one that I have studied for many years, viz., Torbay. This bay is an inlet rather more than 4 miles in breadth and over 3 in depth, carved out of Devonian and Triassic rocks of varying degrees of hardness, and open to the south-east. In its centre there is a level area of about 5 square miles, round which a line can be drawn so as to include every 6-fathom sounding and to exclude every other. The bottom over this area consists superficially of a very fine sand, of which a sample taken at any spot will represent the whole. After heavy easterly gales, as has been already stated, the water is very turbid, and the slushy bottom occasionally becomes harder. The level surface of the bottom, the uniformity of its material, the alteration in its character after gales, and the turbidity of the water, all point to one conclusion, viz., that storm waves materially affect the bed of the bay. To the intensity of this action the fauna also bears witness. Shells that inhabit the 5- and 6-fathom areas, such as *Thracia convexa* and *Cardium aculeatum* are occasionally washed ashore from considerable distances. A valve of a full-grown *Thracia convexa*, picked up on Paignton Sands, was some 3,000 yards distant from the only spot where, to my knowledge, that mollusc has been taken alive in Torbay. Specimens of *Cardium aculeatum* are occasionally washed ashore and sometimes in vast numbers, but they are invariably denuded of their spines. Even though not washed ashore, thousands are sometimes rolled and killed in the 6-fathom area, whilst those that survive testify to the severity of the ordeal passed through by the damage done to their shells, and by the repairs effected. The contrast between the old shell denuded of spines and the rim of new growth with spines perfect is often very marked. In one specimen in the museum of the Torquay Natural History Society half the shell is quite smooth and the other half furnished with perfect spines. There are very few genera of molluscs, whether bivalve or univalve, that inhabit the 6-fathom area of Torbay, provided their shells are not internal, whose shells do not occasionally bear upon them the marks of a struggle for existence, more or less severe, with the storm waves of Torbay.

The marks of damage to which I allude, when severe, cannot be mistaken for lines indicating cessation of growth from change of temperature, lack of food, or other such cause; they do not indicate merely a check in the formation of new shell, but in very many cases the destruction of the old. Nor are these marks confined to individual shells alone, for they are often common to whole colonies together. If a single cardium be found, with the new shell growing out from under the old, owing to the edges having been too much damaged to admit of continuous shell formation, it may be contended that the individual mollusc had met with some special accident, but when hundreds of cockles are dredged together, showing the same marks of damage, repair, and subsequent growth, it is impossible to escape from the conclusion that they were all subjected together to some serious disturbance of their beds.

In a paper read to the Devonshire Association, in 1878,* I showed, from the data furnished me by Lord Rayleigh, that on the 6-fathom area in Torbay a wave 300 feet long and three feet above mean level, if such ever occurred, would cause an alternating current at the bottom with a maximum speed of 3 feet per second. On two occasions (22nd October, 1880, and 4th April, 1881), since then, during easterly gales, I have observed waves with a period of $8\frac{4}{7}$ seconds, and on one occasion, viz., on the 19th October, 1881, waves with a period of 8 seconds. On the last occasion, as has been already stated, I was able, by means of known marks to measure both the wave-length and speed. The length where measured proved to be 275 feet and the speed 660 yards per minute, but as the waves, before reaching the shore had to traverse about 1,000 yards of water less than 6 fathoms in depth, their length in midbay probably did not fall short of 300 feet. Their height I had no means of measuring, but at the low computation of one-thirtieth the wave-length, it would be 10 feet, or 5 feet above mean level.†

Leaving Torbay, with its comparatively shallow water, I will proceed to examine the evidence of disturbance at the bottom, in the deeper waters of the English Channel. The evidence at hand is of a varied nature, and includes the testimony borne by the character of the bottom itself, by valves of shells and other inanimate objects dredged up, by the character of the fauna, and by experienced fishermen.

In April, 1880, a large earthenware jar was brought up in the trawl of the Brixham smack "Pelican" about 20 miles south-east of the Start point, where the depth, according to the chart, is about 36 or 37 fathoms.

Mr. Pengelly, F.R.S., has described this jar with its contents (as received at Torquay) of half-a-pint of sand and gravel, and from the fact that "the whole surface of the bottom, as well as about fully one-half of the entire lateral surface" was covered with marine organisms and that the jar was not abraded, arrived at the conclusion "that the jar underwent little or no movement after reaching the sea bottom . . . . . that there was very little movement of the gravel there," and that "of storm-wave movement there could have been none, and of tidal-wave movement very little."* This reasoning seemed to me very difficult to turn aside, until I was told by one of the crew of the "Pelican" that the jar contained a quantity of gravel, and that it could scarcely have moved, being so weighted. This point seems to me of so much importance that I recently requested Mr. Hayden, the captain of the "Pelican," who did not remember the circumstance, to make further enquiry of the second hand, who had given me the information. He replied as follows:—

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* "Trans. Dev. Assoc.," vol. x, p. 192.

† On the 25th October, when the weather had moderated, H.M.S. "Inflexible" left Plymouth for Gibraltar. On her arrival there, Captain Fisher reported having encountered waves 300 feet in length and 24 feet in height.—("Western Morning News.")

"Brixham, Feb. 16th, 1882.

"DEAR SIR,—I have been speaking to Mr. Dyer about the jar that we caught, and he says he remembers very well about it, and that it was nearly full of very dirty gravel; and I think the jar did not move on the bottom; when the bottom gets disturbed it must have washed in the jar. Please to excuse my writing to you only I thought you would like to know. I have got a few shells, and I hope by the time Mr. Baynes comes over again I shall have a basketful.—From J. HAYDEN, Master of smack 'Pelican.'

"I think the gravel must have been in the jar a very long time, owing to its being so dirty."

I have ascertained by measurement that when the jar is laid on its side, the lower internal lip of the neck is 9½ inches above the surface on which the jar is resting. The internal diameter of the neck is less than 2 inches. Through this small hole the gravel that filled it must have found its way; but for it to do this, it was absolutely necessary, either by the motion of the empty jar, or of the gravel, or of both, to get rid of the 9½ inches of space that, during times of quiescence, lay between them.

Then, again, when found, the jar was half buried in gravel, and this fact also proves sufficient motion at the bottom in 36 fathoms of water to move gravel whose character has been described by Mr. Pengelly, from the sample left in the jar, as "sub-angular and rounded stones, the largest of which scarcely exceeded a hazel nut in size." The fact that the jar was half-buried is of importance, as it proves motion of the bottom itself, and is not liable to the objection that might possibly be raised (though there is strong evidence to the contrary) that the jar was full of gravel when lost.

The evidence of motion afforded by the character of the marine fauna, if considered in detail, would require more space than can be afforded in the present paper. A very cursory glance at it must suffice.

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* "Trans. Dev. Assoc.," vol. xii, p. 76.

The character of the fauna of the littoral zone is such that it can be seen at a glance that the chief enemy that has to be contended against is the wash of the waves. Molluscs and crustaceans living on rocks are specially adapted to cling tightly to those rocks, whereas those living on sand have the power of burrowing in the sand. Where the ground is solid, as in the case of rocks, the animals living on it trust to their powers of holding on or of boring into it; where the ground is unstable the animals that frequent it trust to their powers of rapidly shifting their positions. Of the former class the limpet is a good example; of the latter the common razor fish. But if the mollusca of the littoral zone are specially adapted to resist the wash of the waves that would drive them high and dry on shore, it is equally true that many of those living in the laminarian and coralline zones are wonderfully provided against their special danger, viz., the alternate swing of the waves on the bottom. Living as they do continuously under water, their shells are free to assume the most elaborate sculpture and form, from which the littoral shells are precluded, owing to a compulsory cessation of growth twice a day by retreat of the tide. In many cases the development of the lip, or of the sculpture in the form of spines, supplies exactly what the animal wants, viz., a broad base for a sandy bottom. By the kindness of the Rev. A. Cook I have been able to experiment with a few winged and spined shells from different parts of the world. One experiment with a *Murex monodon* from Australia, a *Pteroceras lambis* from the East Indies, a *Strombus tricornis* from the Red Sea, and four specimens of *Aporrhais pes pelecani* from Torbay was very instructive. Placing them all on their backs in my tank, I succeeded on one occasion in fifteen seconds in restoring them all to their proper positions, simply by swinging the water in the tank. Owing to the weight of the foreign shells some difficulty was experienced in getting them in motion, and moreover, with the exception of the *Aporrhais* and *Pteroceras*, they were not particularly suitable for the experiment, as they had not wings or spines very largely developed.

In the case of the extremely long spined murex, *M. tenuispina*, though the spines offer great resistance to the animal being overturned, they do not afford any assistance to the animal to recover its balance when once it has lost it. In the best examples of winged and spinous shells, such as *Aporrhais* and *Pteroceras*, the alternate current requisite to upset them is very much more powerful than one sufficient to restore them to their normal position.

As *Aporrhais pes pelecani* is a beautiful instance of a gasteropod proof against moderate wave action, so the common *Pecten maximus* is a good example of like protection among the bivalves. Owing to one valve being flat and the other curved it follows that a slight disturbance of the water will place it in a stable position. I find that it is quite easy to roll over a full grown *P. maximus* in my small tank if resting on the convex valve, but to dislodge it when resting on the flat valve transcends the power of any current I can bring to bear upon it. Owing to the rejection by naturalists of the theory of submarine wave-motion, the fact that certain parasitic sea anemones, such as *Adamsia palliata* and *Sagartia parasitica*, commonly choose a shell tenanted by a hermit crab (*pagurus*) has been a matter of some perplexity. But, given the submarine wave action, and the problem finds its solution. The crab keeps the shell from rolling, and the anemone from being killed. I have taken many young specimens of *Sagartia parasitica* on living shells of *Turritella terebra*, but from the state in which I have seen shells of this mollusc damaged by rolling, I cannot conceive the possibility of the young anemones having much chance of surviving the first severe storm. The protection afforded by hermit crabs is no matter of fancy, as anyone can see by gently rocking the water in an aquarium, tenanted by hermit crabs, on a sandy bottom. If the crab happens to be in his shell, the first impulse is to dart out his legs and claws, and hold on to the sand on as broad a base as possible. His cousin, the swimming crab (*portunus*), under similar circumstances will burrow, or, if finally dislodged by the shifting of the sand, will dart upwards into the water to escape the commotion. Many of the small fishes, crustaceans, and molluscs that frequent the 6-fathom area of Torbay seem quite on their guard, and prompt in their action when disturbed by oscillating currents in a small aquarium.

Want of space precludes the possibility of pursuing this branch of my subject further, and compels me to turn to the next question, viz., the evidence afforded by the shells of molluscs of motion on the sea floor.

I have assumed that wave action on the bottom of Torbay will scarcely be denied, and have passed lightly the evidence of the Torbay shells. It now remains to consider the evidence of those found in deeper waters. Among the shells sent me by fishermen who have taken them on the oyster ground off the mouth of Torbay, in about 15 fathoms, have been several specimens of *Trochus granulatus*, an inhabitant of the coralline zone. On a careful examination of eight of these shells, it appears that not one of them has escaped rough treatment more than once in the course of its life, and that one of them has had to repair serious damages nine times, over and above any slight abrasion that did not suffice to interfere with the sculpture and regular growth of the shell. On the 10th February, 1882, I bought three scallops (*Pecten maximus*) at a fishmonger's shop, where I was informed they had all been taken off Berry Head on the previous day. Two of them were nearly the same size and showed several marks of interrupted growth; one of the marks was quite unmistakeable, when the shells were about $1\frac{1}{2}$ inches long. I measured each of the convex valves of these pectens independently, recording the size of each when the interruptions to growth occurred. The lengths of each were as follows, measured in inches and sixteenths :—

	No. 1.		No. 2.
Growth checked when the length of shell was	$1\frac{7}{16}$	..	$1\frac{1}{2}$
” ” ”	$2\frac{10}{16}$	..	$2\frac{15}{16}$
” ” ”	$3\frac{1}{8}$	..	$3\frac{1}{4}$
” ” ”	$3\frac{1}{2}$	..	$3\frac{5}{8}$
” ” ”	$3\frac{11}{16}$	..	$3\frac{13}{16}$
” ” ”	4	..	4
” ” ”	$4\frac{1}{4}$	..	$4\frac{5}{16}$
Total length	$4\frac{5}{16}$	..	$4\frac{7}{16}$

The interest in these pectens lies in the correspondence of their lines of arrested growth, which have clearly been caused by some external agent common to both shells, and are not due to any idiosyncracy. Taken separately both shells are inferior as examples of arrested growth to the specimen of *Pecten maximus* figured by Mr. Jeffreys on Plate XXV of his "British Conchology."

On the 3rd of March I received the parcel of shells referred to in Hayden's letter. They were taken whilst the crew of the "Pelican" were pursuing their ordinary avocation of fishing, and their collection was spread over a considerable time. The exact locality whence came each shell cannot of course be specified, but it so happens that with the shells I received fragments of three of the Channel stones, which the crew of the "Pelican" have been in the habit of sending me for some years past. These stones give us a clue as to the depth of water where the vessel had been fishing. One stone was taken 18 or 20 miles S.S.E. of the Start, another 20 miles S. of the Eddystone, and the third 15 miles S.E. of the Start. The depth of water at the places indicated is (according to the chart) 38, 41, and 36 fathoms respectively, giving an average of over 38 fathoms. The majority of these shells bear on them marks of arrested growth, not, as a rule, so decided as those from shallower water, but in many cases quite unmistakeable. Some details of the collection are given in the following table :—

	Showing arrested growth.	Not showing arrested growth or doubtful.	Total number of specimens.
Pecten opercularis	22	3	25
Penna rudis	6	..	6
Cardium echinatum	2	..	2
Cardium norvegicum	2	2	4
Cyprina islandica	2	2	4
Tapes virgineus	1	..	1
Solen ensis	..	1	1
Capulus hungaricus	8	6	14
Trochus granulatus	2	1	3
Trochus zizyphinus	1	..	1
Turritella terebra	..	1	1
Natica catena	..	1	1
Buccinum undatum	4	2	6
Fusus gracilis	3	5	8
Fusus buccinatus	..	2	2
Scaphander lignarius	5	1	6
	58	27	85

Without pretending to affirm that all the cases of arrested growth are due to wave action, it seems to me a significant fact that out of a miscellaneous parcel of shells from the Channel fishing ground, 68 per cent. should show signs of damage caused by some agent external to themselves. Notes on the above shells would be out of place here, but it may be pointed out that the oftentimes damaged state of such a sedentary mollusc as *Capulus hungaricus* may be owing to the fact that it is frequently found attached to pinna, and that pinna is one of the shells frequently found damaged. The frontispiece to Mr. Gwyn Jeffreys' second volume of his "British Conchology" depicts a pinna that has received a decided check to its growth, but by no means a severe one.

Having shown that disturbance of the bottom is evidenced at depths of a few fathoms by the visible turbidity of the water, and at greater depths by the damage done to shells and by the special provision in their habits and structure to withstand such action, I now turn to the evidence of nautical men as to the existence of submarine wave action, and as to the actual dangers to which vessels are exposed owing to the effects of such action. My first witness will be G. Hayden, the skipper of the Brixham trawler "Pelican," and though his evidence amounts to little more than the expression of his opinion, it is the opinion of a man thoroughly acquainted with the subject under consideration. I submitted to him the following queries, to which he appended his replies and attested them by his signature :—

(1.) Do you think the bottom on the fishing grounds off the Start is affected by heavy gales ?—Yes; I think the ground is very much disturbed by heavy gales.

(2.) Do you find the fish act differently after gales, *i.e.*, swim higher or lie closer ?—I think that fish are affected during the gale, but that after the gale they resume their usual habits.

Captain Kiddle, of the White Star steamer "Celtic," writes ("Nat.," vol. 13, p. 108) that "On George's Shoals, off Nantucket, during a heavy gale, the New York pilots and masters of coasting vessels assert that sand is frequently left on deck after a sea has broken on board, although the depth of water may be 12 or 14 fathoms. . . . . The shortness of the sea on the banks of Newfoundland, where the soundings are from 30 to 50 fathoms, is noticed by all the navigators of the Western Atlantic, as it reduces the speed of an ocean steamer more than the heavier waves of deeper water will do. . . . . In the gulf stream north of the Straits of Bemine, after a "norther" has blown a few hours, the surface of the sea is covered with lanes of weed, although only a few patches might have been seen before the commencement of the gale."

Before passing on, it will be well to point out that the disturbance of the bottom on the banks of Newfoundland has strong zoological evidence in its favour. Mr. J. Gwyn Jeffreys, F.R.S., writes as follows of the bivalve *Mya truncata.* "The cod on the North American fishing banks seem to be equally fond of this mollusc; but it is not so easy to say how they procure it. *Mya truncata* is often buried from 8 to 10 inches below the sea-bottom; and it does not seem to be capable of changing its habitation."*

Now, I have taken this very mollusc alive in Torbay after easterly gales, and I have taken flat fish that have been feeding on *Cardium aculeatum* killed by heavy seas. If channel seas can dislodge *Mya truncata* from its deep burrow in Torbay, there is every probability that Atlantic seas will dislodge it occasionally on the banks of Newfoundland. The seaman's assertion solves the problem that has perplexed the naturalist, and the fact observed and recorded by the naturalist strongly corroborates the statement and conclusion of the sailor.

The evidence hitherto adduced goes far to prove that at depths of about 40 fathoms in the English Channel and of 50 on the Banks of Newfoundland there is not only motion at the bottom, but strong motion, far exceeding the gentle oscillation of the water that is sufficient to ripple a sandy sea-bed. According to Sir Charles Lyell, quoting from the "Encyclopædia Britannica," a current of but 6 inches per second will suffice to raise fine sand.† This no doubt refers to constant currents, as it is far in excess of what is sufficient in the case of alternate currents. I found on trial with a small glass aquarium, containing fine blown sand, that an alternating current passing over a space of 2 inches 120 times per minute strongly rippled the sand with ripples varying in size from one inch downwards. Thus an average speed of 4 inches per second sufficed to form ripples so large as one inch from ridge to ridge. It is clear that a much slower current would still suffice to produce smaller ripples.

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* "British Conchology," vol. iii, p. 69.

† "Principles of Geology," vol. i, p. 348, 10th edition.

It would be a matter of great interest to know at what depth an Atlantic wave would set up an alternating current of 4 inches per second, for, whatever the depth, it would fall short of that at which ripplemarks might be formed.

Although the question of the relation of current action to depth of water, and to height and length of wave, is one that must be left to the mathematician, and is one with which I cannot pretend to grapple, the following experiments, though on a small scale, may be worth recording:—

Dried peas placed on a glass plate in a slight depression on a sandy bottom in 6-inch water were rolled off by waves about 12 inches long, and about 1 inch high. Although the motion was due to the waves, the fact that the peas were ultimately rolled off the plate was due to the difficulty of getting the glass perfectly level under water. Shorter waves $1\frac{1}{2}$ inches high had much less effect on them. A little sand that had collected on the glass was beautifully rippled with $\frac{1}{8}$ inch ripples; these were dried and varnished. As it was difficult to discern slight motion of any object owing to the undulation of the water, I proceeded to make a rough indicator, whereby a vane of thin wood placed at the bottom at right angles to the course of the waves would communicate a multiplied motion to a long light needle above the surface.

With this rough machine I tried the following experiments:—

(1.) Water, 8 inches; to top of vane, 7 inches. Waves, 90 per minute; motion by indicator very marked.

(2.) Waves, 115 per minute, half inch high; motion doubtful.

(3.) Waves, 86 per minute, 1 inch high; motion at bottom very strong; index striking both checks.

(4.) Waves, 80 to 90 per minute $\frac{3}{8}$ of an inch high; motion at bottom strong.

This last experiment was an unintentional one. The larger waves had made a strand for themselves, and I found that by reducing the displacement of the wave generator, I could make smaller waves that, without altering the mean level of the water, rippled the lower part of the former strand which they by their smaller reflux failed to uncover. Thus by a slight alteration of adjustment of the generator, I could, by varying the height of the waves, form or destroy ripplemarks at pleasure. Whilst making these experiments, I chanced to reduce the number of the low waves to between 80 and 90 per minute. These, though barely over $\frac{1}{4}$ of an inch in height, immediately affected the submerged vane, and the motion of the index attracted my eye, though I was not attending to it. On a subsequent occasion I found that waves between 80 and 90 per minute, and only $\frac{1}{8}$ of an inch high, moved the index at a depth of $6\frac{3}{4}$ inches, and moved flocculent matter on the sandy bottom at a depth of $7\frac{3}{4}$ inches.

In this experiment motion at the bottom was obtained when the depth was 62 times the height of the wave, though small in proportion to the wave-length.

If the evidence of the existence of alternate currents on the floor of shallow seas is strong; the evidence that the currents of power sufficient to roll and damage shells, are not constant currents, is still stronger. Molluscs, such as *pinna*, and *mya*, that have little, if any, powers of locomotion, could not coexist with currents capable of transporting in any quantity, even fine sand. They would perish, either from the destruction of their beds by the removal of the sand, or from fresh material being piled on top of them. Mr. Godwin Austen has well said, that a "drift-sand zone" is wholly unfitted for marine life.* But it is the drifting sand that is fatal, not the mere fact that the drift sand zone is the one that "comes within the range of the tidal and wave disturbance of the water." Off Paignton Sands in Torbay, the very zone described by Mr. Godwin Austen abounds in *Cardium tuberculatum* and *Donax vittatus*, but owing to the fact that the sands are open to heavy seas from one quarter only, they cannot drift, and the shells mentioned, as a rule survive the attacks of the waves, though many individuals succumb.

As ripple marks are formed under water, so also they can be preserved under water; and they are more likely to be there preserved than on a sea beach, where on the retreat of the tide, they are liable to be effaced by the very swell that has formed them.

In the case of a lake or sea, subject to slight changes of level, with a river running into it carrying mud or sand, we have all the conditions necessary for the formation and preservation of ripplemark. An occasional swell from the lake will ripple the submerged sand in the vicinity of the accumulating deposits brought down by the stream, and these will quietly cover up the ripples without effacing them. A slight rise in the level of the lake will shift the area of deposition further up the stream, and the bed that covered the ripples will in course of time be itself rippled, and in its turn covered up. In this manner a series of ripplemarks and beds of mud or sand often "false-bedded," may be rapidly formed, and in the case of increase of current as rapidly destroyed.

Ripples that have been left bare by the retreating tide, or possibly by the sinking waters of an inland lake or sea, may often be distinguished from those that have been formed and preserved under water. The former are usually imperfect through loss of their sharp ridges.* Occasionally in addition to this they form natural channels for surface drainage, and whilst their ridges are levelled, their furrows are deepened. This fact seems to be referred to by Dr. Geikie, in the third edition of his "Manual of Geology," where he points out, that owing to the ridges of fossil ripplemarks being often broad and equable while the intermediate furrow has a little channel; a cast can be distinguished from an original rippled surface, by the channel in the original surface producing a sharp little crest on the cast ("Man. Geol.," Jukes and Geikie, p. 172, 3rd Ed.).

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* "Natural History of the European Seas," p. 233.

The points that I have endeavoured to establish in the foregoing pages may be briefly recapitulated as follows:—Marine ripplemarks are formed by alternate currents set up by waves. Experiments with short high waves on a small scale prove strong action at a depth of half the wave-length, whilst the evidence of the marine fauna, and the testimony of nautical men go far to prove that ocean and channel waves strongly affect the sea bottom to at least the same relative depth. The depth at which proof of wave action can be forthcoming falls far short of that at which fine deposits can be rippled by currents incapable of leaving permanent traces in the damage done to shells.

It is scarcely necessary to point out that the subject treated of in the present paper, namely, the formation of ripplemark, is only one branch of a far wider and more important one, which for the sake of brevity, I have as much as possible kept in the back ground, namely, that of submarine denudation.

II. "Note on General Duane's Soundless Zones." By Dr. JOHN TYNDALL, F.R.S. Received March 21, 1882.

In reference to one of the powerful fog-whistles established on the coast of Maine, General Duane remarks as follows:—"The most perplexing difficulties, however, arise from the fact that the signal often appears to be surrounded by a belt varying in radius from 1 to 1½ mile from which the sound appears to be entirely absent. Thus, in moving directly from a station the sound is audible for the distance of a mile, is then lost for about the same distance, after which it is again distinctly heard for a long time. This action is common to all earsignals, and has been at times observed at all the stations, at one of which the signal is situated on a bare rock 20 miles from the mainland, with no surrounding objects to affect the sound."

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* For an illustration of the former class see plate facing page 170 of "The World's Foundations," by Miss Agnes Giberne; and, for an illustration of the latter, see page 19 of Sir Charles Lyell's "Elements of Geology," 6th Edition. A.R.H., 4th May, 1882.

For a long time past, I have thought that this disappearance of the sound was due to the interference with the direct waves, of waves reflected from the surface of the sea. This explanation is capable of very accurate experimental illustration. Placing, for instance, a sensitive flame at a distance of 3 or 4 feet from a sounding reed, the flame exhibits the usual agitation. Lifting a light plank between the flame and reed, a position is easily attained where the sound, reflected from the plank, increases the flame's agitation. Lifting the plank cautiously still higher, a level is attained, reflection from which completely stills the flame. By slightly raising or lowering the plank, or by its entire removal, the flame is once more agitated. In these experiments a high pitched reed was used, so that it was easy to produce by the motion of the plank the retardation of half a wave-length requisite for interference.

In General Duane's case, a fairly smooth sea would be required for the reflection; while the position of the zone of silence would be determined by the height of the signal on the one hand and the height of the observer on the other above the surface of the sea. The position would also, of course, depend on the pitch of the note of the whistle.

The preparation of some lectures on the "Resemblances of Sound, Light, and Heat," has recently brought this subject to mind, hence the present communication.

April 27, 1882.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The Right Hon. Lord Bramwell and Dr. Ramsay H. Traquair were admitted into the Society.

The following Papers were read:—

I. "On the Attitudes of Animals in Motion." By EADWEARD MUYBRIDGE, of Palo Alto, California. Communicated by THE PRESIDENT. Received April 20, 1882.

II. "Preliminary Account of the Structure of the Cells of the Liver, and the Changes which take place in them under Various Conditions." By J. N. LANGLEY, M.A., Fellow of Trinity College, Cambridge. Communicated by Dr. MICHAEL FOSTER, Sec. R.S. Received April 24, 1882.

I have examined the structure of the liver-cells in the frog, toad, newt, the common snake, the grey lizard, the roach and smelt, the pigeon, and various mammals. In all these the "resting" liver cells have the following common points of structure.

The protoplasm is arranged in the form of a network or honeycomb, the meshes or spaces of which are in all parts of the cell of much the same size; the outer parts of the cell are formed of a thin layer of slightly modified protoplasm with which, however, the network is continuous.

The spaces of the protoplasmic network are occupied by paraplasm or interfibrillar substance, consisting of (1) spherical granules, probably proteid in nature; (2) spherical globules of fat; (3) hyaline substance, filling up the spaces not occupied by the granules or globules. This substance consists partly of glycogen and in part probably of a proteid.

This description differs in several points from those given by previous observers. The account given by Klein* resembles it more nearly than any other. Klein described the liver-cells of mammals as consisting of a protoplasmic network and of a hyaline interfibrillar substance; any apparent granules present he considered to be nodal points of the network. In preparations made after Klein's methods the granules are in fact with difficulty, or not at all, to be made out. They are, however, very obvious in fresh teased or in osmic acid specimens, especially in such specimens of the liver of the mole.

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* Klein, "Quart. Journ. Med. Sc.," N.S., xix, p. 161, 1879.

Previous to Klein's account of the mammalian liver-cells, Kupffer* described the liver-cells of the frog as consisting of a protoplasmic network and of hyaline paraplasm. He figures the network as being very irregular, with small granules in its bars, as being completely absent from considerable portions of the cell, and as having much finer meshes in the outer part of the cell around the nucleus. Kupffer's observations were made upon the tissue treated with osmic acid or with 10 per cent. salt solution and iodine. In sections of a frog's liver hardened in osmic acid I find the cells to consist of a slightly stained mass, in which granules and fat globules are imbedded. Treatment with other reagents shows that this slightly stained mass really consists of a network, and interfibrillar substance, the latter not in a granular form, but occupying the spaces in the network unoccupied by the granules and globules. If the liver is exposed for some time before it is placed in osmic acid, the granules are no longer distinct; an irregular network-like structure is formed out of them. This may possibly be the network of Kupffer. Further, in the liver-cells of a healthy, hungry (not fasting) frog the protoplasmic network stretches fairly equally throughout all parts of the cell; in winter frogs which have long fasted (cf. below, p. 22) the network has wider spaces and thinner bars in one part of the cell than in the other; but then the wider meshes are found in the outer, and not in the inner part of the cell and are not absent from any cell region. Lastly, in osmic acid specimens of such cells the outer parts appear homogeneous, except for fat globules, the inner zone is crowded with distinct granules.

Hence it is obvious that the network of protoplasm and the paraplasm described by Kupffer do not in the least correspond to the network of protoplasm and interfibrillar substance described by myself. Whilst the liver-cells of all classes of vertebrates which I have examined have the above-mentioned common characters in the "resting" state, they have certain minor distinguishing characters in each class, depending chiefly upon the size of the cells and their nuclei, the position of the nuclei, and the relative amount of the various cell-constituents present. Of these, however, I do not propose to give an account here.

Hering, in 1867, pointed out that the liver in all vertebrates except mammals is an anastomosing tubular gland. Hence we should expect that the changes which take place during digestion in the liver of an amphibian, reptile, fish, or bird should differ somewhat from those which take place during digestion in the liver of a mammal. I have taken the liver of the frog and toad as an example of the tubular gland form, and have made observations on this during the last two years and a half. How nearly the changes which take place in other "tubular" livers resemble those which take place in the "tubular" livers of the frog and toad can of course only be decided by direct observation; but it seems unlikely that the change should take any very different course, since the resting state of the cells and their arrangement in the gland is so very nearly the same.

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* Kupffer, "Festschrift an Carl Ludwig," 1875.

The Liver of the Frog.

The liver of the frog and that of the newt can be observed while the blood continues to circulate through it; in this state nothing is seen in the cells except the fat globules; if a small piece be teased out in salt solution, the faintly outlined granules are seen floating in the fluid; these become obvious on adding iodine. The best method for bringing out the granules and fat globules is to place a small piece of the liver in osmic acid, 1 per cent., for a day, to transfer it to alcohol for several days, and then to prepare sections; in such sections the granules and globules appear to be imbedded in homogeneous cell substance. The granules are largest in the liver of the newt, so that the liver of this animal is best adapted to show that the granules are not the nodal points of the network. The network of the cell is brought out most distinctly by chromic acid, 0·5 per cent.; it can also be seen in specimens hardened in picric acid, alcohol, mercuric chloride, or in teased out specimens of a liver which has been treated with neutral ammonium chromate, 5 per cent.

When a section of the liver which contains glycogen, and which has been hardened in acohol or osmic acid, is placed for some minutes in iodine solution, certain parts of the interfibrillar substance of the liver-cells stain red-brown. The substance which is so stained I conclude to be glycogen. By water, even at 30° C., it is only slowly dissolved. Since isolated glycogen is readily soluble in water, this might lead us to think that the red-staining substance is not glycogen; but we know that glycogen is not readily extracted from the liver by warm water, and hence it seems probable that a large part of the glycogen of the liver exists in it in a form not very soluble in water; but, whatever may be the condition of glycogen in the liver, I think I am justified in concluding that the red-staining substance in alcohol specimens is glycogen, since glycogen can be extracted from them, since the amount of the red-staining substance varies directly with the amount of glycogen which can be extracted, since the coloration is just that produced when iodine is added to a little purified glycogen on a glass slide, and since it is rapidly dissolved by amylolytic ferment, such as an aqueous or glycerine extract of the parotid of a rabbit.

During the summer months the liver of a hungry frog has granules scattered equally throughout the cell, and there is very little glycogen. During the long winter fast the cells change in appearance; the granules become more and more confined to the inner part of the cell, and form there a marked inner granular zone. The glycogen increases in amount, and is stored up chiefly in the outer part of the cell, where there are no granules. Osmic acid specimens of glands in this condition show two distinct zones, an inner granular one and an outer, apparently homogeneous, one; the nucleus lies at the border of the two, or if the outer zone is large lies in it alone. When such a specimen is treated with iodine, all or nearly all of the outer zone stains red-brown; around the granules also a varying amount of red-brown stained substance is found; the network of the inner zone, the granules, and the nucleus stain yellow. In these specimens the network of the outer part of the cell cannot at all, or only very imperfectly be made out. It is, however, seen in sections of the gland which have been hardened in chromic acid. It is continuous with the network of the inner part of the cell, but has wider spaces, and its bars are finer.

We know that in the œsophageal glands of the frog and in such gastric glands as have been investigated on the point, the changes which take place in the cells in fasting closely resemble the changes which take place in them during digestion. So here, in the liver of the frog, the changes which take place when the animal is fed closely resemble those gradually established during the winter fasting period. The extent of the changes occurring in digestion depends greatly upon the state of the liver cells before the animal is fed; in summer the changes are slight, there is only a slight decrease of granules in the outer part of the cell and a slight increase of glycogen. The changes are much more marked when the cells have to start with a small outer non-granular zone; in such cases in the 6th to 8th hour of digestion the outer zone is large, and in the 24th to 30th the cells become granular throughout. When a frog which has already a large outer non-granular zone is fed the decrease of granules usually lasts a shorter time, and in the 6th to 8th hour of digestion the granules begin to increase. In other words, the using up and formation of granules go on at different relative rates in different nutritive conditions of the body.

The disappearance of granules and the formation of glycogen which takes place in winter frogs is only partly brought about by the absence of food; it is brought about in part perhaps chiefly by the low temperature. If winter frogs, the liver-cells of which have few granules and much glycogen, are kept at about 20° C. for a week to a fortnight, the cells become granular throughout and the glycogen largely disappears; similarly frogs in spring or autumn, the liver-cells of which have many granules and little glycogen, if kept at a low temperature a week to a fortnight, present in the cells of the liver an outer non-granular zone and an increase of glycogen; in summer frogs the effect is much less. Further, the changes during digestion are slight in winter frogs that have been kept in the warm, greater in spring and autumn frogs which have been kept in the cold.

Although, generally speaking, a decrease of granules goes hand-in-hand with an increase of glycogen and an increase of granules with a decrease of glycogen, yet a certain amount of variation in the one may take place without any variation or any corresponding variation in the other. Hence I regard the formation of granules and the formation of glycogen as independent processes. A comparison of the changes which take place in the granules of the liver-cells with the changes which take place in the granules of the salivary, gastric, and pancreatic glands leaves me with no doubt that the granules of the liver-cells are destined to give rise to some constituent or constituents of the bile, and it seems to me more than probable that by appropriate chemical treatment, we may obtain from them some one or more of the constituents of the bile-salts.

In the account I have just given I have omitted all mention of the fat globules. These vary so much with different conditions of the body, as yet unknown, that it is extremely difficult to determine what changes in their number and position take place during digestion. It is well known that there is in winter an increase in the amount of fat in the liver. Generally speaking, in summer the fat globules are small, few, and fairly equally scattered throughout the cell, with a tendency to be more numerous around the lumen. In winter frogs the greatest variation occurs, occasionally there are very few, usually there are a considerable number, not unfrequently the cells are crowded with them, this is generally the case with obviously unhealthy frogs. Further, in the most common condition, that in which the fat globules are fairly numerous without being crowded together in the cells, they may occur almost entirely in the inner or almost entirely in the outer part of the cell. In the former case they make a conspicuous fat globule zone about the lumen, in the latter they occur in conspicuous clumps close to the outer cell-border; this is generally the case when one has reason to suppose that fat is increasing in the cells. If winter frogs are kept in the warm, the fat globules diminish in number; if summer frogs are kept in the cold for a week to a fortnight there is a slight, but only a slight, increase in the quantity of fat. The majority of frogs which are fed in the summer show little or no change in the number or size of fat globules in the liver.

In winter the amount of fat in the liver varies so much in frogs apparently alike, that I do not feel justified in drawing any conclusions as to the changes occurring in digestion.

The only other vertebrates with liver of the tubular type on which I have made a series of observations, are the toad and newt. The changes in the liver of the toad are in all essential respects the same as those I have described for the liver of the frog. In the newt's liver I have not observed the formation of zones during digestion; it appears to me to depart largely from the tubular type of gland, and to resemble in structure rather the mammalian than the ordinary amphibian liver. In the snake during the winter's fast, an outer non-granular zone makes its appearance in the liver tubules; it is, however, much smaller than that in the frog and toad.

I may mention that the cells of the frog's bile-duct, where it runs through the pancreas, are ciliated; the pancreatic duct with conciliated cells joins it close to the small intestine.

The Mammalian Liver.

In the mole the granules of the liver are conspicuous, and are preserved by osmic acid; in the dog, cat, and rabbit the granules are more or less altered, hence I have chosen the mole to make observations upon as to the changes which take place in digestion. In the hungry animal the protoplasmic network stretches throughout the cells with nearly equally sized meshes; in the spaces of the network is a small amount of hyaline substance, partly glycogen, and embedded in this are rather large granules. When the liver is examined six to eight hours after digestion, there is a greater or less disappearance of granules from the centre of the cell around the nucleus; the network here has wider spaces and thinner bars, and the spaces are for the most part filled with glycogen. In cases where these changes are most marked, osmic acid specimens treated with iodine show a diffuse reddish stained mass surrounding the nucleus; at the borders of the cell, the yellow stained network is seen, and one or two rows of granules; between these a little red stained substance may usually be traced continuous with the central mass of glycogen; the peripheral network and granules make the cell appear at first sight as if it had a very thick cell wall. The network in the central part of the cell is brought out by hardening a piece of the liver in chromic acid and other reagents. In cells in which the digestive changes are less advanced, the glycogen may only partially surround the nucleus, or may be accumulated more on one side of it than elsewhere.

Histological observations on the increase of glycogen in the liver-cells have been made by Bock and Hofmann,* on the rabbit, and by

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* Bock u. Hofmann, "Virchow's Archiv.," 56, s. 201, 1872.

Kayser,* on the dog. Bock and Hofmann found that glycogen accumulated in amorphous form around the nucleus, and stretching out from this as a network towards the periphery of the cell. My own observations confirm this in the main. I regard, however, the glycogen as being stored up in the spaces of a protoplasmic network. Bock and Hofmann also mention granules of the liver-cells which stain yellow with iodine,† but they do not appear to have distinguished between these and the protoplasmic network which likewise stains yellow with iodine.

Kayser (*loc. cit.*) found that glycogen was stored up in the form of "Schollen oder Körner;" these lumps and granules of glycogen I have not seen; an appearance simulating this occurs when the glycogen is stored up more in some parts of the protoplasmic network around the nucleus than in others, but these local collections never have, so far as I have observed, sharply marked boundaries. The granules are not distinguished by Kayser from the protoplasmic network; these two together make up, I imagine, the thick peripheral layer of protoplasm described by him as occurring in cells which contain much glycogen. As in the liver of the frog, so in the mammalian liver, I take the granules to be concerned in the formation of some of the substances found in the bile.

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* Kayser. Quoted by Heidenhain. "Hermann's Hdb.," Bd. v, Th. 1, s. 221, 1880.

† Similar granules were also described by Plosz. "Pflüger's Archiv.," vii, s. 371, 1873.

May 4, 1882.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

In pursuance of the Statutes, the names of Candidates recommended for election into the Society were read from the Chair, as follows:—

Ball, Professor Valentine, M.A.
Brady, George Stewardson, M.D., F.L.S.
Buchanan, George, M.D.
Clarke, Charles Baron, M.A., F.L.S.
Darwin, Francis, M.A., F.L.S.
Dittmar, Professor William, F.C.S.
Gaskell, Walter Holbrook, M.D.
Glazebrook, Richard Tetley, M.A.
Godman, Frederic Du Cane, F.L.S.
Hutchinson, Professor Jonathan, F.R.C.S.
Liversidge, Professor Archibald, F.G.S.
Malet, Professor John C., M.A.
Niven, William Davidson, M.A.
Palgrave, Robert Henry Inglis, F.S.S.
Weldon, Walter, F.C.S.

The following Paper was read:—

I. “On the Specific Resistance of Mercury.” By Lord Rayleigh, F.R.S., Professor of Experimental Physics in the University of Cambridge, and Mrs. H. Sidgwick. Received April 24, 1882.

(Abstract.)

The observations detailed in the paper were made with the view of redetermining the relation between the B.A. unit and the mercury unit of Siemens, *i.e.*, the resistance of a column of mercury at 0°, one metre in length, and one square millim. in section.

According to Siemens' experiments,

1 mercury unit $=\cdot 9536$ B.A. unit,

and according to Matthiessen and Hockin,

1 mercury unit $=\cdot 9619$ B.A. unit.

The value resulting from our observations agrees pretty closely with that of Siemens. We find

1 mercury unit $=\cdot 95418$ B.A. unit.

Four tubes were used to contain the mercury, of lengths varying from 87 to 194 centims.. The diameter of the three first tubes was about 1 millim. and that of the fourth about 2 millims. The final numbers obtained from the several fillings of the tubes are as follows :—

Tube I........	·95386 ·95412 ·95424 ·95436 ·95421	Mean ·95416
Tube II.......	·95389 ·95414 ·95437 ·95436	Mean ·95419
Tube III......	·95424 ·95418 ·95399 ·95425	Mean ·95416
Tube IV	·95440 ·95415	Mean ·95427

Combining the results of the present paper with our determination of the B.A. unit in absolute measure, we get

$$1 \text{ mercury unit} = \cdot 94130 \times 10^{9} \text{ C.G.S.}$$

May 11, 1882.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The Right Hon. Sir Henry Bartle Edward Frere (elected 1877) was admitted into the Society.

The following Papers were read :—

I. "Sur l'Inversion Générale." Par T. S. VANĚČEK. Communicated by Dr. HIRST, F.R.S. Received April 27, 1882.

Dans une note "Sur l'inversion générale" qui était publiée dans les Comptes rendus de l'Académie Française,* j'ai exposé l'idée d'une transformation plus générale que celle par rayons vecteurs réciproques. Dans la note présente je ferai l'extension de cette transformation.

1. Considérons une conique générale C et deux droites L, D. La première doit être transformée par rapport à la droite D, appelée directrice, et à la conique C, courbe fondamentale.

A un point a de la droite L correspond une polaire A par rapport à C et coupe la droite D en un point a_1. La polaire A_1 de ce point coupe la droite A en un point a_2 qui est le transformé du point a.† Sa polaire passe par les points a et a_1. Les points a, a_1, a_2 forment un triangle polaire par rapport à la conique fondamentale C.

Quand le point a parcourt la droite proposée L, le point a_1 parcourt la droite directrice D, et le point a_2 décrit une conique (a_2) qui passe par les points d'intersection des deux droites L, D avec la conique C et par leurs pôles l, d.‡ Sa polaire A_2 enveloppe une autre conique (A_2) qui touche les droites données L, D et les tangentes à la conique fondamentale dans les points d'intersection de celle-ci avec les droites L, D.

2. Supposons que la droite L soit remplacée par une courbe d'ordre m et la droite D par une courbe d'ordre n, la courbe inverse de l'une

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* Tome xciv, p. 1042 (10 Avril, 1882).

† Hirst, "On the Quadric Inversion of Plane Curves," "Roy. Soc. Proc.," (1865), vol. 14, p. 92.

‡ *Ibid.*, p. 95.

par rapport à l'autre est d'ordre $2mn$, donée de $2n$ points d'ordre m et de $2m$ points de l'ordre n.

Nous pouvons donc énoncer les théorèmes suivants :

Quand le sommet a *d'un triangle polaire* a, a_1, a_2 *par rapport à une conique* C *parcourt une courbe* L *de l'ordre* m *et* a_1 *parcourt une courbe* D *de l'ordre* n, *le troisième sommet* a_2 *décrit une courbe d'ordre* 2 mn *qui a* 2m *points multiples d'ordre* n *et* 2n *points multiples d'ordre* m *qui sont les points d'intersection des courbes* L, D *avec la conique* C. Et réciproquement.

Quand le côté A *d'un triangle polaire par rapport à la conique* C *enveloppe une courbe* L_1 *de la classe* m, *et le côté* A_1 *enveloppe une courbe* D_1 *de la classe* n, *le troisième côté* A_2 *enveloppe une courbe* (A_2) *de la classe* 2mn, *douée de* 2m *tangentes multiples de la classe* n *et de* 2n *tangentes multiples de la classe* m *qui sont respectivement tangentes communes des courbes* L_1, C *et des courbes* D_1, C.

Les courbes (a_2) et (A_2) sont les polaires réciproques par rapport à la conique C.

3. La courbe (a_2) est la même si nous transformous la courbe L par rapport à la directrice D ou cette courbe D par rapport à L comme directrice. Considérons la courbe L comme dans le paragraphe précédent, d'ordre m, et la courbe D d'ordre n. Les points d'intersection de la courbe L ou D avec la conique fondamentale C appelons les points fondamentaux. Nous pouvous énoncer les théorèmes suivants.

Un simple point fondamental de la courbe L devient un point multiple d'ordre n de la courbe inverse (a_2).

Le point fondamental d'ordre m_1 de la courbe L est un point multiple d'ordre $m_1 n$ de la courbe inverse (a_2).

Quand les deux courbes L, D ont un point fondamental simple a commun, ce point se transforme en un point multiple d'ordre $m+n-2$ de la courbe (a_2) et en la tangente de la conique fondamentale C en ce point a, qui fait une partie de la courbe inverse (a_2).

Le point fondamental a étant un point multiple d'ordre m_1 de la courbe L et le point multiple d'ordre n_1 de la courbe D, ce point se transforme en un point multiple d'ordre.

$$(n-n_1)m_1+(m-m_1)n_1,$$

et en la tangente A de la conique fondamentale en ce point ; la droite A fait une partie de la courbe inverse (a_2) et elle est une droite multiple d'ordre $m_1\, n_1$.

Le point multiple a d'ordre m_1 de la courbe L n'étant pas un point fondamental se transforme en n points multiples d'ordre m_1 qui se trouvent sur la droite A, polaire du point a. Quand ce point a se réunit avec un point multiple d'ordre n_1 de la courbe D, il se transforme en m points multiples d'ordre n_1 de la courbe inverse (a_2), qui sont distribués sur la même droite polaire A.

D'après cela il est toujours possible de déterminer le nombre et l'espèce des points multiples de la courbe inverse et aussi l'ordre de la courbe (a_2) ou la classe de la courbe (A_2).

Supposons que la courbe inverse (a_2) d'une courbe L par rapport à une autre courbe D comme directrice soit construite; la courbe inverse (α_2) de la courbe (a_2) par rapport à L se décompose en la courbe D et, une autre courbe, dont l'ordre est déterminé, ou réciproquement la courbe (a_2) a pour courbe inverse par rapport à la courbe D directrice une courbe qui se décompose en la courbe L et en une autre courbe, dont l'ordre est connu.

4. Le point a de la courbe proposée L a une tangente A' a cette courbe; cette tangente coupe la conique fondamentale C en deux points t, u. La polaire A du point a coupe la courbe directrice D en n points. La tangente B' en un de ces points b rencontre C en deux points x, y, et la droite polaire B du point b coupe la droite A en un point a_2 qui est un point de la courbe inverse (a_2). Considérons le point de contact a comme deux points infiniment voisins; la même chose a lieu au point b. A ces points infiniment voisins correspondent aussi tels points dans la courbe inverse.

Ainsi la courbe inverse de l'une des droites A', B' par rapport à l'autre, qui est une conique E, a avec la courbe (a_2) deux points infiniment voisins communs. Ces deux courbes ont par conséquent une tangente commune en ce point a_2. La conique E est plus que déterminée par les points t, u, x, y et par a_2. Quand les points t, u ou x, y sont imaginaires, ils sont remplacés par la tangente et son point de contact, ou respectivement par deux tangentes et leurs points de contact et par le point a_2.

Le point a_2 sur la courbe inverse (α_2) étant donné nous construisons sa droite polaire par rapport à la conique fondamentale C et cherchons le point a sur L et b sur D qui correspondent au point a_2. La conique E est alors déterminée et par conséquent aussi la tangente en a_2.

II. "On the Organisation of the Fossil Plants of the Coal-measures. Part XII." By Professor W. C. WILLIAMSON, F.R.S. Received May 4, 1882.

(Abstract.)

At the recent meeting of the British Association at York, Messrs. Cash and Hick read a memoir, since published in Part IV of vol. vii of the "Proceedings of the Yorkshire Geological and Polytechnic Society," in which they described a stem from the Halifax Carboniferous deposits characterised by a form of bark hitherto unobserved in those rocks. To this plant they gave the name of Myriophylloides William-

sonis. It was characterised by having a large cellular medulla, surrounded by a thin vascular zone composed of short radiating lamellæ. This, in turn, was invested by a cylinder of cortical parenchyma from which radiated a number of thin cellular laminæ, like the spokes of a wheel, separating large lacunæ. Each lamina generally consisted of a single series of cells. At their peripheral end, these laminæ merged in a thick, large-celled, cortical parenchyma. The generic name, Myriophylloides, was given to the plant because of the resemblance between sections of its cortical tissues and those of the recent Myriophyllum. Two reasons induced the author to object to this name ("Nature," December 8, 1881, p. 124), and to propose the substitution of that of Helophyton. Such substitution, however, was rendered unnecessary by the discovery, by Mr. Spencer, of Halifax, of some additional specimens which indicate that the supposed new plant was merely the corticated state of the Astromyelon, described by the author in his Memoir, Part IX.* These specimens showed that the plant was more complex than had been supposed, different ramifications of it having individual peculiarities.

In some of the new specimens the vasculo-medullary axes present no differences from those of the Astromyelon already described. The radiating lines of cells separating the lacunæ prove to be transverse sections of elongated vertical laminæ composed of cells with a mural arrangement, and which separate large vertical lacunæ of varying lengths; a type of cortical tissue clearly indicating a plant of aquatic habits. So far as this bark is concerned, all the ramifications of the plant display similar features, but several of the specimens exhibit important variations in the structure of the vasculo-medullary axis. In them the central cellular medulla is replaced by an axial vascular bundle, which has little, or in some examples apparently no, cellular element intermingled with the vascular portions. In some examples this axial bundle is invested by the thick exogenous zone seen in Astromyelon. In others that zone is wholly wanting. Yet there appears to be no reason for doubting that these are but varied states of the same plant which branched freely, the differentiated branches having, doubtless, some morphological significances, as yet incapable of being explained. That the plant was a Phanerogam allied to Myriophyllum is most improbable. It has several features of resemblance to the Cryptogamic Marsileæ, from which it does not differ more widely than the fossil Lepidodendra do from the living Lycopodiaceæ.

The author describes a new specimen of Psaronius Renaultii, found by Mr. Wild, of Ashton-under-Lyne. Those previously described consisted almost entirely of fragments of the bark and its ærial rootlets. The present specimen contains a perfect C-shaped fibro-vascular bundle and a portion of a second one, resembling some of those described by Corda, and which leave no room for doubting that our British Coal-measures contain at least one arborescent fern, equal in magnitude to those obtained from the deposits at Autun.

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* "Phil. Trans.," 1878.

In his Memoir, Parts IX and X, the author described under the provisional generic name of Zygosporites, some small spherical bodies with furcate peripheral projections. Similar bodies had been met with in France, and were regarded by some of the French palæontologists as true Carboniferous representatives of the Desmidiaceæ. The author was unable to accept this conclusion, deeming it much more probable that they would prove to be spores of a different kind. Mr. Spencer exhibited the specimen now described at the York meeting. It is a true sporangium, containing a cluster of these Zygosporites. Though they undoubtedly bear a close superficial resemblance to the zygospores of the Desmidiæ, their enclosure within a common sporangium demonstrates them to be something very different. There is now no doubt but that they are the spores of the strobilus described by the author in his Memoir, Part V, under the name of Volkmannia Dawsoni. Hence the genus Zygosporites may be cancelled.

Another interesting specimen found by Mr. Wild is a young Calamite, with a more curiously differentiated bark than any that has hitherto been discovered. The structure of the vascular cylinder and of the innermost layer of the bark differs in no essential respect from those previously described; but the outermost portion displays an entirely new feature. It consists of a narrow zone of small longitudinal prosenchymatous bundles, each one having a triangular section, the apex of each section being directed inwards, whilst their contiguous bases are in contact with what appears to be a thin epidermal layer. As in every previously discovered Calamite in which the cortex is preserved, the peripheral surface of this specimen is perfectly smooth or "entire." It displays no trace of the longitudinal ridges and furrows seen in nearly all the traditional representations of Calamites figured in our text-books.

It has long been seen that the medullary cells of the Lepidodendra, as well as the vessels of their non-exogenous medullary sheaths, steadily increased in number as these two organs increased in size correlatively with the corresponding general growth of the plants. But the way in which that increase was brought about has continued to be a mystery. The author now describes a Lepidodendron of the type of L. Harcourtii in which nearly every medullary cell is subdivided into two or more younger cells, showing that, when originally entombed, the pith was an extremely active form of meristem, though the branch itself had attained to a diameter of at least two inches. The numerous small young cells are of irregular form. Their development by further growth into a *regular* parenchyma would inevitably necessitate a corresponding increase in the diameter of the branch as a whole;

and it must have been from these newly-formed cells that the medullary cylinder obtained the elements out of which to construct the additional vessels, the increase of which has been shown to be the invariable accompaniment of the growth of the branch. As might be expected, the growth of the vascular cylinder or medullary sheath could only have been a centripetal one.

A new form of Halonia from Arran is described. Instead of its central portion consisting, as in previously described examples, of the usual Lepidodendroid medulla, surrounded by a vascular cylinder, it consists of a solid axis of vessels, resembling in this respect all the very young Lepidodendroid twigs previously described from the same locality. Many recently obtained specimens of Lepidodendroid branches sustain the author's previous observations that all examples from Arran having less than a certain diameter have the solid axial bundle; whilst all above that diameter have a cylindrical vascular bundle enclosing a cellular medulla. The first type commences with the smallest twigs, and is found increasing gradually up to the diameter referred to. The second type begins where the other ends, and increases in diameter until attaining the dimensions of the largest stems, in none of which does the solid bundle reappear. Halonial branches have not hitherto been described attached to the branches of any true Lepidodendron, though, in 1871 (Memoir, Part II), the author gave reasons, based upon organisation, for insisting that Halonia was a fruit-bearing branch of a Lepidodendroid tree. This conclusion was sustained by Mr. Carruthers in 1873 in his description of a branch belonging to a Lepidophloios. The author now figures a magnificent example, from the museum of the Leeds Philosophical Society, of a dichotomous branch of a true Lepidodendron of the type of L. elegans and L. Selaginoides. In this specimen every one of the several terminal branches bears the characteristic Halonial tubercles. The leaf-scars of these latter branches have the rhomboid form once deemed characteristic of the genus Bergeria, whilst those of the lower part of the specimen are elongated as in L. elegans, &c. These differences are not due to their appearance in separate cortical layers of the branch, but to the more rapid growth in length of its lower part compared with its transverse growth.

The author throws some additional light upon the structure of Sporocarpum ornatum described in Memoir, Part X, as also upon the nature of the development of the double leaf-bundles seen in transverse sections of the British Dadoxylons, described in Memoir IX. After a prolonged but vain search for a similar structure amongst the twigs of the recent Conifers, the author has at length found them in the young twigs of the Salisburia Adiantifolia. Sections of these twigs made immediately below their terminal buds exhibit this geminal arrangement in the most exact manner. Pairs of foliar bundles are given off from the thin, exogenous, Xylem zone which encloses the medulla, whilst at the same points the continuity of the Xylem ring is interrupted, as was also the case with the Dadoxylons, by an extension of the medullary cells into the primitive cortex. Sections of the petiolar bases of the leaf-scales of the bud show that these bundles enter each petiole in parallel pairs, subsequently subdividing and ramifying in the Adiantiform leaf. This curious resemblance between Salisburia and Dadoxylon, accompanied as it is by other resemblances in the structure of the wood, bark, and medulla, suggest the probability that our British Dadoxylon was a Carboniferous plant of Salisburian type, of which Trigonocarpum may well have been the fruit. If so, the further possibility suggests itself that this plant may have been the ancestral form whence sprang the Baieras of the Oolites, and, through them, the true Salisburias of Cretaceous and of recent times.

The Society adjourned over Ascension Day to Thursday, May 25.

May 25, 1882.

THE PRESIDENT in the Chair.

The Presents received were laid on the table and thanks ordered for them.

Mr. Bindon Blood Stoney was admitted into the Society.

The following Papers were read:—

I. "On certain Geometrical Theorems. No. 2." By W. H. L. Russell, F.R.S. Received May 10, 1882.

(4.) The following is a short method of determining the conic of 5 pointic-contact with a given curve. Write the conic

$$\alpha y+\beta xy=y^2+\mu x^2+\nu x+\rho \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1),$$

then differentiating four times and writing D for $\frac{d}{dx}$, we have, remembering that the four first differential coefficients of the two curves coincide,—

$$\alpha \mathrm{D}y+\beta \mathrm{D}(xy)=\mathrm{D}y^2+2\mu x+\nu \quad . \quad . \quad . \quad . \quad . \quad . \quad (2),$$

$$\alpha \mathrm{D}^2y+\beta \mathrm{D}^2(xy)=\mathrm{D}^2y^2+2\mu \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3),$$

$$\alpha \mathrm{D}^3y+\beta \mathrm{D}^3(xy)=\mathrm{D}^3y^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4),$$

$$\alpha \mathrm{D}^4y+\beta \mathrm{D}^4(xy)=\mathrm{D}^4y^2 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5),$$

whence
$$\alpha=\frac{D^3y^2D^4(xy)-D^4y^2D^3(xy)}{D^3yD^4(xy)-D^4yD^3(xy)},$$

and
$$\beta=\frac{D^3y^2D^4y-D^4y^2D^3y}{D^3(xy)D^4y-D^4(xy)D^3y},$$

then μ, ν, ρ are found from (1), (2), (3), and the conic determined.

(5.) Let us now endeavour to show how the equation to the cubic with 9-pointic contact with a given curve is to be found.

Writing the cubic

$$ay^2x+bx^2y+cy^2+dy+exy=y^3+\mu x^3+\nu x^2+\rho x+\sigma,$$

we differentiate eight times and obtain five equations from which μ, ν, ρ, σ have disappeared.

$$aD^4(y^2x)+bD^4(x^2y)+cD^4y^2+dD^4y+eD^4(xy)=D^4y^3,$$

with four others obtained by substituting in this equation D^5, D^6, D^7, D^8 successively for D^4. Hence we shall have—

$$a.\begin{Bmatrix} D^4(y^2x), & D^4(x^2y), & D^4y^2, & D^4y, & D^4(xy) \\ D^5(y^2x), & D^5(x^2y), & D^5y^2, & D^5y, & D^5(xy) \\ D^6(y^2x), & D^6(x^2y), & D^6y^2, & D^6y, & D^6(xy) \\ D^7(y^2x), & D^7(x^2y), & D^7y^2, & D^7y, & D^7(xy) \\ D^8(y^2x), & D^8(x^2y), & D^8y^2, & D^8y, & D^8(xy) \end{Bmatrix}$$

$$=D^4y^3\begin{Bmatrix} D^5(x^2y), & D^5y^2, & D^5y, & D^5(xy) \\ D^6(x^2y), & D^6y^2, & D^6y, & D^6(xy) \\ D^7(x^2y), & D^7y^2, & D^7y, & D^7(xy) \\ D^8(x^2y), & D^8y^2, & D^8y, & D^8(xy) \end{Bmatrix}$$

$$+D^5y^3\begin{Bmatrix} D^4(x^2y), & D^4y^2, & D^4y, & D^4(xy) \\ D^6(x^2y), & D^6y^2, & D^6y, & D^6(xy) \\ D^7(x^2y), & D^7y^2, & D^7y, & D^7(xy) \\ D^8(x^2y), & D^8y^2, & D^8y, & D^8(xy) \end{Bmatrix}$$

$$+D^6y^3\begin{Bmatrix} D^4(x^2y), & D^4y^2, & D^4y, & D^4(xy) \\ D^5(x^2y), & D^5y^2, & D^5y, & D^5(xy) \\ D^7(x^2y), & D^7y^2, & D^7y, & D^7(xy) \\ D^8(x^2y), & D^8y^2, & D^8y, & D^8(xy) \end{Bmatrix}$$

$$+D^7y^3\begin{Bmatrix} D^4(x^2y), & D^4y^2, & D^4y, & D^4(xy) \\ D^5(x^2y), & D^5y^2, & D^5y, & D^5(xy) \\ D^6(x^2y), & D^6y^2, & D^6y, & D^6(xy) \\ D^8(x^2y), & D^8y^2, & D^8y, & D^8(xy) \end{Bmatrix}$$

$$+D^8y^3\left\{\begin{matrix} D^4(x^2y), & D^4y^2, & D^4y, & D^4(xy)\\ D^5(x^2y), & D^5y^2, & D^5y, & D^5(xy)\\ D^6(x^2y), & D^6y^2, & D^6y, & D^6(xy)\\ D^7(x^2y), & D^7y^2, & D^7y, & D^7(xy) \end{matrix}\right\}.$$

In the same way b, c, d, e are determined, and then μ, ν, ρ, σ are known from the third, second, first, and original equations.

(6.) In the same way we may proceed to find the 14-pointic contact of a curve of the fourth order. The coefficients will be expressed by series of determinants, each having eight rows and eight columns, divided by a determinant having nine rows and nine columns. And the same method will apply to the general case.

II. "Note on Mr. Russell's paper 'On certain Geometrical Theorems. No. 2.'" By WILLIAM SPOTTISWOODE, Pres. R.S. Received May 25, 1882.

If we apply Mr. Russell's formulæ to the determination of the sextactic points of a curve, we shall have, in addition to the equations (1)—(5), the following—

$$\alpha D^5y+\beta D^5(xy)=D^5y^2\,;$$

and by elimination of α and β from his equations (4) and (5), together with this latter, we shall obtain as the condition for a sextactic point—

$$\begin{array}{lll} D^3y, & D^3xy, & D^3y^2=0 \quad \ldots\ldots\ldots \quad (A).\\ D^4y, & D^4xy, & D^4y^2,\\ D^5y, & D^5xy, & D^5y^2. \end{array}$$

But

$$\begin{aligned} Dxy &= y +xDy,\\ D^2xy &=2Dy +xD^2y,\\ D^3xy &=3D^2y +xD^3y,\\ D^4xy &=4D^3y +xD^4y,\\ D^5xy &=5D^4y +xD^5y,\\ Dy^2 &=2yDy,\\ D^2y^2 &=2yD^2y+ 2(Dy)^2,\\ D^3y^2 &=2yD^3y+ 6DyD^2y,\\ D^4y^2 &=2yD^4y+ 8DyD^3y+ 6(D^2y)^2,\\ D^5y^2 &=2yD^5y+10DyD^4y+20D^2yD^3y. \end{aligned}$$

Substituting in equation (A), we obtain—

$$\begin{array}{llll} D^3y, & xD^3y+3D^2y, & 2yD^3y+\ 6DyD^2y & =0, \\ D^4y, & xD^4y+4D^3y, & 2yD^4y+\ 8DyD^3y+\ 6(D^2y)^2, & \\ D^5y, & xD^5y+5D^4y, & 2yD^5y+10DyD^4y+20D^2yD^3y. & \end{array}$$

Whence by obvious inductions—

$$\begin{array}{llll} D^3y, & 3D^2y, & & =0 \\ D^4y, & 4D^3y, & 3D^2y, & \\ D^5y, & 5D^4y, & 10D^3y. & \end{array} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (B).$$

Also $D\{4(D^3y)^2-3D^2y\,.\,D^4y\}=4D^3y\,.\,D^4y-3D^2y\,.\,D^5y.$

Hence the equation (B) may also be written thus—

$$(10D^3y-3D^2y\,.\,D)\{4(D^3y)^2-3D^2y\,.\,D^4y\}=0 \quad . \quad . \quad (C).$$

And in order to evaluate the expression it will consequently be necessary, in the first instance at least, to calculate only $Dy, \ldots D^4y$.

The main interest in the problem, however, lies in the determination of the degree of this equation, which has been found by Cayley and myself to be $12n-27$. The difficulty lies in getting rid of the extraneous factors. In order to effect this, the expression (C) must be transformed by the substitution of $-u:v$ for Dy, and other expressions to be calculated for $D^2y, \ldots D^4y$.

If $U=0$ be the equation to the curve, and if we adopt a usual notation, and put u, v for the first, and u_1, w', v_1 for the second differential coefficients of V with respect to x and y; also if

$$(v\delta_x-u\delta_y)'^2U=v^2u_1-2vuw'+w^2v_1,$$

and similarly for higher powers, *i.e.*, if the operative factor $(v\delta_x-u\delta_y)'$ is understood to affect U only, precisely as if u, v, &c., were constant; then it is not difficult to show that—

$$\begin{aligned} -Dy\ &=u:v, \\ -D^2y&=(v\delta_x-u\delta_y)'^2U:v^3, \\ -D^3y&=(v\delta_x-u\delta_y)'^3U:v^4-3(vw'-wv_1)(v\delta_x-u\delta_y)'^2U:v^5, \\ -D^4y&=(v\delta_x-w\delta_y)'^4U:v^5-4(vw'-wv_1)(v\delta_x-u\delta_y)'^3U:v^6 \\ &\quad -3\{2v(v\delta_x-u\delta_y)'^2-5(vw'-uv_1)^2-v^2(u_1v_1-w'^2)\}(v\delta_x-u\delta_y)^2U:v7. \end{aligned}$$

These are the developments by means of which the reduction would have to be made.

III. "On Effects of Retentiveness in the Magnetisation of Iron and Steel. (Preliminary Notice.)" By J. A. EWING, B.Sc., F.R.S.E., Professor of Mechanical Engineering in the University of Tokio. Communicated by Professor Sir WILLIAM THOMSON, F.R.S. Received May 6, 1882.

The term Hysteresis was introduced in a paper,[*] recently communicated to the Royal Society, to designate a peculiar action which was observed in the inquiry then recorded, and which had also presented itself in an earlier investigation—of the effects of stress on thermoelectric quality.[†] It was found that when a stretched iron wire was gradually loaded and unloaded the changes of thermoelectric quality lagged behind the changes of stress, so that curves exhibiting the relation of stress to thermoelectric quality during the putting on and taking off of the load were far from coincident, but inclosed between them a wide area.[‡]

In prosecuting those experiments it occurred to me that there is much room for investigation of hysteresis[§] in the changes of magnetisation of iron and other substances produced by (1) change of the magnetic field; (2) change of stress; (3) change of temperature. In (2) and (3) two cases are to be considered:—First, when the substance is exposed to a constant magnetising force; second, when the magnetisation which is changed is wholly residual.

From the known character of residual magnetism we may at once infer that when magnetisation along any axis is changed so considerably that its sign is reversed there must be hysteresis, but it is not clear that any such phenomenon need appear when the action is confined to one sign. In fact, Maxwell's extension of Weber's theory of induced magnetism[||] assumes that residual magnetism resembles the "permanent set" of a strained solid, and implies that any subsequent application of a magnetising force in the same direction with and not exceeding that by which the residual magnetism has been produced, will give changes of a quasi-elastic character not exhibiting the action which I have called hysteresis.

By the direct magnetometric method, and also by the ballistic method (as used by Rowland and others), I have examined at great length the changes of magnetisation which occur in iron and steel when the magnetising force is progressively increased, diminished, again increased, reversed, and so on. The results show in the most conclusive manner that all changes of magnetisation produced by slow or fast, continuous or discontinuous, changes of the magnetising force exhibit hysteresis. If we carry the metal through any cycle of magnetisation, the curves giving the relation of I (the intensity of magnetisation) to H (the magnetising force) form loops, and it does not appear that the loops are different in any essential respect (except size) when the action is confined to one sign from the loops given when the sign of the magnetisation is reversed.

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[*] "On the Production of Transient Electric Currents in Iron and Steel Conductors by Twisting them when Magnetised, or by Magnetising them when Twisted." "Proc. Roy. Soc.," vol. 33, p. 21.

[†] "Effects of Stress on the Thermoelectric Quality of Metals. Part I." "Proc. Roy. Soc.," vol. 32, p. 399.

[‡] Since the paper cited was laid before the Royal Society, I have learnt that M. Emil Cohn has anticipated me in the discovery of this peculiar feature of the effects of stress on thermoelectric quality. ("Pogg. Ann.," N.F., VI, 385.)

[§] Or effects of retentiveness—Note by Sir William Thomson, May 5, 1882.

[||] "Treatise on Electricity and Magnetism," II, chapter vi.

The remarkable feature of the curves is, that when the magnetisation of iron is conducted in such a manner as to be uniform throughout the piece experimented on, the initial change which occurs when we pass from increase to decrease of the magnetising force, or *vice versâ*, is indefinitely small relatively to the initial change of the force. In other words, say that we stop decreasing H and begin to increase it, then $\frac{dI}{dH}$ is at first zero.

The difference between the curves for increase and decrease of the magnetic force is of a perfectly static character. If it is to be explained by internal friction, the friction is analogous to that of solids, and does not at all resemble the viscosity of liquids. The phenomenon here described is independent of the quasi-viscous resistance to changes of magnetisation which is due partly to the induction of currents in neighbouring conductors, including the magnet itself, and partly to the thermomagnetic properties of the metal discussed by Sir W. Thomson ("Phil. Mag.," vol. v, 1878, pp. 24–25). The influence of those causes disappears when the changes of magnetisation take place very slowly, or when a sufficient interval of time is left after each change of magnetic force before a reading of the magnetisation is taken.

When any cyclic change of I is made to take place by varying H cyclically, the area of the loop so formed, or $\int IdH$, is not only proportional to, but actually the measure of the work done on the magnet, per unit of volume, in performing the cycle. In cases where changes of the magnetisation take place very slowly this is wholly spent on the magnet itself, and its equivalent is, no doubt, to be found in the heating effect of the cycle. When, however, the changes of magnetisation take place at a finite rate, this area must of necessity be greater, since the work done in performing the cycle is then greater for two reasons; first, because of the energy expended in inducing currents in neighbouring conductors; and, second, because of the dissipation of energy involved in the heating and cooling effects which Thomson has shown must occur on account of the fact that the susceptibility to magnetic induction is a function of the temperature.

I have endeavoured to account for the static hysteresis by supposing that the rotation of Weber's magnetic molecules is opposed by a frictional couple of constant moment, not necessarily the same for all the molecules in a given piece. It seems not unlikely that residual magnetism itself may be due to this frictional sticking of the molecules rather than to the quasi-plasticity suggested by Maxwell. The examination of this theory, as well as the description of the experiments, some of whose results have been briefly mentioned in this notice, will form the subject of a more detailed communication.

Another portion of the work has consisted in looking for hysteresis in the changes of the longitudinal magnetism of iron wires, produced by pulling and relaxing pull, the wires being under the influence of the vertical component of the earth's magnetic force, which in Tokio is about 0·34 C.G.S. unit. Sir W. Thomson* has investigated very extensively the general effects of stress on the magnetisation of iron and other metals, in magnetic fields of various strengths, but without special reference to this point. Only in the case of torsion (alternately to opposite sides) is mention made of any action of the kind which I have termed hysteresis. His researches were for the most part conducted by the ballistic method,† by which the currents induced in a solenoid surrounding the wire were observed when a single weight was put on and taken off. By making several steps, instead of only one, in the application and removal of the load, the existence of hysteresis may easily be demonstrated by this method; but I have preferred the direct magnetometric method, which has the immense advantage of exhibiting the actual magnetic state of the stretched wire at any time.

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* "Electrodynamic Qualities of Metals," "Phil. Trans.," 1856, 1876, 1879.

† [Note by Sir William Thomson of May 3, 1882.—This is not quite so. The experiments described in §§ 214—244 of my "Electrodynamic Qualities of Metals," Part VII ("Phil. Trans." for 1879, p. 55), were performed by the magnetometric method. My earlier experiments described in §§ 178—213 ("Phil. Trans." for 1876, p. 693, and for 1879) were performed by the ballistic method.

The following is taken from a preliminary statement (§ 178) :—"Early in the year 1874, I made arrangements to experiment on the magnetisation of iron and steel wires in two different ways—one by observing the deflections of a suspended magnetic needle produced by the magnetisation to be tested, the other by observing the throw of a galvanometer needle, due to the momentary current, induced by each sudden change of magnetism. The second method, which for brevity I shall call the ballistic method, was invented by Weber, and has been used with excellent effect by Thalén, Roland, and others. It has great advantages in respect of convenience, and the care with which accurate results may be obtained by it; but it is not adapted to show slow changes of magnetism, and is therefore not fit for certain important parts of the investigation. On this account I am continuing arrangements for carrying out the first method, although hitherto I have obtained no good results by it."

The first method was accordingly followed in all the latter part of my experiments on this subject; not only those described in §§ 214—244 referred to above, but also in further investigations which I have continued up to the present time, and of which I hope to offer results to the Royal Society before long.]

Each wire was hung vertically with its upper end on a level with a mirror magnetometer. It was then annealed by heating to bright redness with a spirit-lamp, and after it had become cool, weights were progressively applied.

During the earliest part of the first loading certain very interesting apparently anomalous effects occur, which will be described in the detailed account. Apart from these, which are easily distinguished, the following is the normal action:—

If to the annealed wire any load not exceeding the elastic limit is successively applied and removed (without shock), its application causes a *decrease* and its removal an *increase* of magnetisation. The "on" and "off" curves of stress and magnetism are widely different, and afford an excellent instance of hysteresis.

Next, let the wire be stretched beyond its limit of elasticity. The stretching is accompanied by a decrease of magnetisation, which continues so long as the wire keeps "running down." When the load is removed it is found that a great diminution of magnetisation has taken place; but besides this, the wire has undergone a very remarkable change with respect to its subsequent behaviour under stress.

For let weights now be gradually applied: they cause at first an *increase** of magnetisation, but this passes a maximum and falls off slightly as that value of the load is approached which previously was applied to produce the permanent set. Let the load then be gradually removed: the magnetisation at first increases, passes a maximum (at a considerably lower value of the load than that which gave the maximum during application), and finally diminishes rapidly to its previous value with no load.

These effects will be clearly seen by reference to fig. 1, which shows the results of a small part of one set of observations. The ordinates are proportional to the total magnetisation, and the abscissæ are the loads in kilogrammes. In this case the wire was of moderately soft iron, 0·79 millim. in diameter, and had a well-defined limit of elasticity at about 10 kilos.

The upper part of the diagram shows the effect of gradually applying and removing 7 kilos. before the wire had been stretched at all by any greater load. The lower part shows the effect of applying and removing weights nearly equal in all to 16 kilos. after the wire had been previously stretched by the same load. The initial magnetism for zero stress had then fallen to 104, but during application it rose to 174 with 10 kilos., and again during removal it rose to 172 with 6½ kilos.

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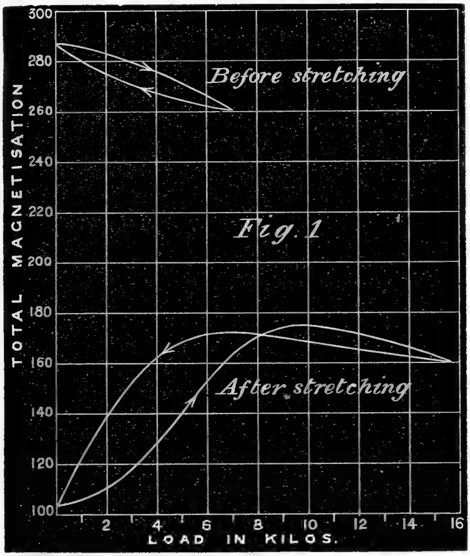
* This agrees with Sir W. Thomson's observation that with low magnetising forces the effect of "on" is to increase, and "off" to diminish magnetism. The description of the wire examined by him shows that it was in fact in the state described in the text. (See "Phil. Trans.," 1879, p. 56.)



The series of experiments from which this figure is taken shows that the maximums of magnetisation during loading and unloading (when conducted without shaking the wire) appear only after some permanent set has been given, and that they gradually shift out to the right as the amount of permanent set is progressively increased.

A number of other iron wires tested in the same way agree with this one in giving *decrease* of magnetism for "on," and *increase* for "off" before stretching beyond their limits of elasticity, and afterwards *increase* for "on" and *decrease* for "off." It appears that (at least when the strain has occurred in the circumstances in which it occurs here) this difference of behaviour forms an unfailing criterion by which we may distinguish a piece which has received permanent set from a piece in the annealed state.

A careful examination of the initial parts of the curves which are formed when after loading we change (without mechanical disturbance) to unloading, or *vice versâ*, has brought out the fact that, calling I the magnetisation and p the stress, $\frac{dI}{dp}$ is always initially zero. The *magnetic rigidity*, as the reciprocal of this differential coefficient might be called, is infinite at the beginning of any change from loading to unloading, or from unloading to loading, provided that the change takes place without agitation of the wire. In the thermoelectric experiments described in my former paper, the beginning of the new curve generally continued to show the same kind of change as had been going on before. The same peculiarity can be reproduced here if the loading or unloading occurs with a slight vibration of the wire, and I now think it almost certain that its presence in the thermoelectric curves was due to a very small amount of mechanical disturbance which accompanied the changes of load.

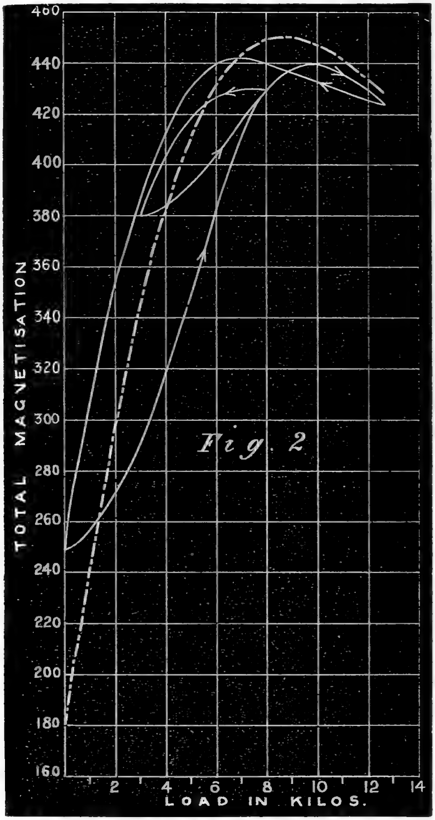


Fig. 2

When the wire is vigorously tapped during the loading and unloading, the "on" and "off" curves so nearly approach coincidence as to lead to the conclusion that a sufficient amount of vibration would destroy the hysteresis altogether. As an instance of this, the full lines in fig. 2 show the changes undergone by another wire (which had been permanently stretched) when subjected to the cycle 0—8—3—12½—0 without vibration; while the broken line is the position in which the "on" and "off" curves very nearly coincided, when the same main cycle 0—12½—0 was passed through with the accompaniment of violent vibration. Its maximum lies, as regards load, between the two previous maximums, and the whole range of magnetic change is considerably increased.

The hysteresis which occurs in the relation of magnetisation to stress is absolutely static. The value of the magnetism associated with any condition (past and present) of stress is reached at once, and remains unchanged for any length of time, when the load is kept constant.

A full account of the experiments will be given when they are more complete. They are being conducted in the Laboratory of the University of Tokio, with the valuable help of the senior students of physics.

IV. "On Actinometrical Observations made in India at Mussooree in Autumn of 1880, and Summer and Autumn of 1881." By J. B. N. HENNESSEY, F.R.S., Deputy Superintendent Great Trigonometrical Survey of India. Received May 2, 1882.

[PLATE 1.]

1. My last communication dealt with the actinometrical observations made by Mr. W. H. Cole, M.A., and myself in 1879; I have now the pleasure to submit the observations taken in 1880 by Mr. Cole, and in 1881 by Mr. H. W. Peychers* and myself. The former happen to be few in number, but the latter present the longest series I have ever been able to take, extending as they do over thirty-two days. The 1881 observations were moreover made under certain special conditions, which are not without interest. Hitherto the two actinometers used (belonging to the Royal Society) were both† of the kind invented by the Rev. G. C. Hodgkinson, and marked by me A and B. One of these was employed at Dehra, the other at Mussooree, the observations being taken as nearly as practicable at the same moments of time; but, as the former of these stations cannot be considered free from objections, which I have discussed in previous communications, I determined to restrict future observations to Mussooree alone. This

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* This being the first occasion of mentioning Mr. Peychers' name, I add, that as he has worked under me for several years, I can vouch for him as an accurate and painstaking observer.

† Both these actinometers are still identical in all respects with their condition when received in 1868.

change, moreover, enabled me to adopt the desirable condition that *two* observers and instruments, *placed side by side*, should work with one and the same chronometer between them, so that two independent results might be obtained simultaneously. It was under these conditions that the *autumn* series of observations in 1881 were made; otherwise, both the summer and autumn series of 1881, as well as the series of 1880, were taken exactly in keeping with the observations of 1879. The procedure followed in the last-named series is fully described in the "Proceedings of the Royal Society," vol. 31, p. 154, to which I may be permitted to refer, instead of making repetitions here, especially since my leisure is at present limited, so that it had perhaps better be devoted to what follows.

2. Returning to the autumn simultaneous observations of 1881, not only were *two* actinometers employed side by side, but the two instruments were of *different* patterns. One of them was A (Hodgkinson's), which has been amply described in my previous communications; the other instrument was one* of those constructed according to designs by Professor Balfour Stewart. It is marked No. 2, and is supplied with two similar tubes, or thermometers, viz., No. 1306 and No. 1307, both of which read from 0° to 100° F., and are graduated on the glass to degree fifths. Tube 1306 alone was employed; it was readily adapted to give readings for so long a time as thirty consecutive minutes of observation, by casting off sufficient mercury into the pear-shaped receptacle above. This was done once for all before the series was begun, and in fact the surplus mercury is still in the receptacle. Otherwise, particular attention was paid to the settings of the thermometer, in rotation and depth, and to those of the lens; the latter was kept scrupulously clean, and was used to its full extent, *i.e.*, no stops were employed. It will thus be seen that the observations were all strictly differential. The graduations were read as usual with a magnifying-glass; the exposures were of sixty seconds alternately in sun and shade, with thirty seconds' intervals between, so that the instruments were read *both at the beginning and end of each sixty seconds' exposure*, all of which is in keeping with what was done during previous work. In point of observers, Mr. Peychers worked A and I used No. 2.

3. My observations before 1881 were restricted to the autumn, but an opportunity occurred in the summer of 1881, which was utilised so long as it lasted. The results are of an unexpected nature.

4. There is another point to be mentioned. Increased experience has suggested keener discrimination in respect to atmospheric conditions, and this has called for some concise mode of indication, which I

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* For the loan of this instrument I am indebted to the kindness of H. F. Blanford, Esq., F.R.S., Meteorological Reporter to the Government of India.

designate "day letters." These I briefly explain hereafter, first reiterating that I never observe when any *visible* interpositions exist between the sun and me; and, in fact, what can be the use of measuring solar radiation through a visible varying atmospheric umbrella, such as is represented by cloud, mist, haze, dust, and smoke? I thought of using a Crookes' radiometer (happening to possess one) as a pioneer to the actinometer, in order to see if the former would suitably indicate when the latter may be employed to good purpose, but the intention unavoidably fell through.

5. Now, in respect to my day letters, the cases I had to provide for were these: the two *visible* causes of interposition are cloud (including mist) and haze (including dust, smoke, &c.). First as to cloud:—

(1.) The whole visible sky may be perfectly clear, or there may be half-a-dozen small patches here and there, but none within 50° or 60° of the sun; for this state my day letter is A.

(2.) But sometimes the condition A required qualifying, because of a peculiar cloud behaviour. The sun being due south, small cloud balls, some 2° to 5° in diameter, appeared on the horizon about north-west, and gradually rolled up and eastwards to some 30° short of the sun, *where they became invisible.* When the circular track, if continued in imagination, passed well *under* the sun, I indicated this by B.

(3.) But if the track passed *through* the sun, the day letter used is C.

6. Then as regards haze: I stood on the outermost mountain range, south of which lie the plains and to the north successive hill ridges, ending in the perpetual snows. No haze is generated *on* the hills apparently, but is blown up from below, the wind being southerly as a rule: hence

(4.) For no haze south or north I use *a*.

(5.) For haze to south only, *b*.

(6.) When the haze is both south and north, I infer that I stand in it though it is invisible overhead; for this I use *c*.

Hence, by using suitable combinations of these letters, the condition of the sky is fairly indicated symbolically, at least for Mussooree.

7. The present series have been reduced in exactly the same manner as those of 1879 were reduced. The *individual* results are attached, viz.:—

For 1880 Autumn, October 18th to 27th, in T. 1, by Mr. W. H. Cole, M.A.

For 1881 Summer, April 25th to May 9th, in T. 2, by Mr. H. W. Peychers and myself.

For 1881 Autumn, October 11th to November 11th, in T. 3, by Mr. H. W. Peychers and myself.

In these tables the results of A and B are expressed in terms of A, glass off; for those of No. 2 the unit is 0°·01 F., assuming a graduation of No. 2 to be 0°·2 F.

8. It needs but little familiarity with the two kinds of instruments to decide in favour of that by Professor Balfour Stewart over that by the Rev. G. C. Hodgkinson, a preference which may be greatly increased by doubling the sensitiveness of the former. I have accordingly converted *all* my results beginning with 1869, and expressed them in terms of Stewart's No. 2, or 0°·01 F., which unit is to be understood *in all that now follows.*

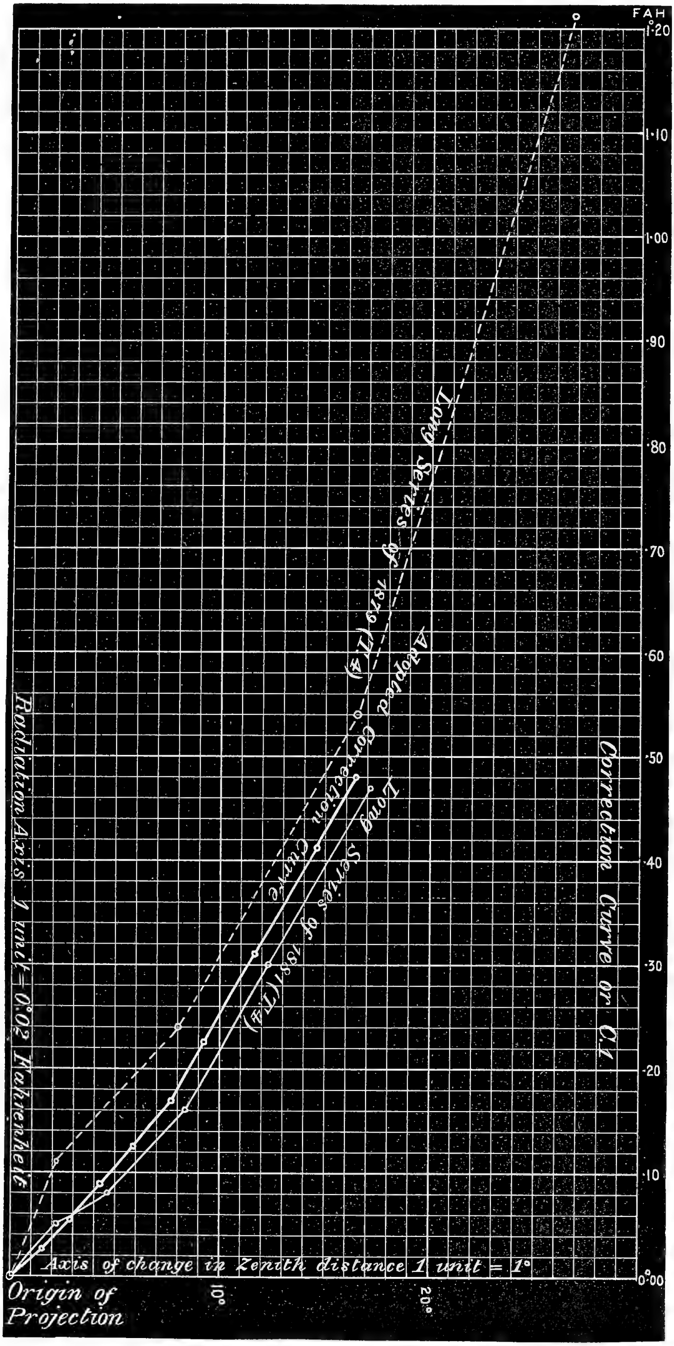
9. Again, as the average zenith of distance of the sun during my autumn series does not differ greatly one year with another, and as suitable corrections to an adopted zenith distance may be inferred from the long range series (*i.e.*, three or four hours on each side of the meridian) taken at the same season of the year, I have reduced *all* my *autumn* results to the zenith distance of 45°.

10. The long range series used are given in T. 4, where it will be seen that the series with A in 1881 is inconsistent, so that I have rejected it. It appears a waste of time to attempt formulating at present the correction from one Z.D. to another; I made several endeavours of the kind with the long series of T. 4, but feeling that the expressions were more pretentious than real, I resorted to the projection shown in the correction Curve C 1: here it will be seen that the two curves, *i.e.*, of 1879 and 1881, are fairly *similar*, and as the series of 1881 was taken at half-hour intervals instead of the one-hour intervals of 1879, the correction curve I have adopted is made to agree more nearly with the later series. All the reductions to 45° Z.D. have been made with the help of C 1.

11. From T. 4, long series, rejecting the results of A, 1881, we have—

App. time.	Defect from noon.	
h.	1879.	1881.
8	115	
9	45	39
9. 5	..	26
10	19	15
10. 5	..	6
11	6	4
1	15	6
1. 5	..	11
2	30	18
2. 5	..	25
3	62	55
4	127	

where the 1879 series is *hourly*, and from 8 A.M. to 4 P.M., while the


FAH
1°·20
1·10
1·00
·90
·80
·70
·60
·50
·40
·30
·20
·10
0°·00
Long Series of 1879 (T.4)
Adopted Correction Curve
Long Series of 1881 (T.4)
Correction Curve or C.1
Radiation Axis 1 unit = 0°·02 Fahrenheit
Axis of change in Zenith distance 1 unit = 1°
Origin of Projection
10°
20°

1881 series is *half*-hourly, and from 9 A.M. to 3 P.M. Taking sums of the defects at the common hourly points, 9h. to 3h., we have—

	1879.	1881.
Defect before noon	70	58
„ after „	107	79
Radiation greater *before* noon	37	21

where the two series agree in showing that the radiation *before* noon is in *excess*, as pointed out before. And as respects differences between defects from noon at corresponding hour angles, we have—

Comparing.	Hour ∠.	Difference of defect. 1879.	1881.	Mean.
8 and 4	4	12	..	12
9 „ 3	3	17	16	16
9.5 „ 2.5	2.5	..	9	9
10 „ 2	2	11	3	7
10.5 „ 1.5	1.5	..	5	5
11 „ 1	1	9	2	5

where the value at ∠ 4 alone is contradictory, but the results at so large an hour ∠ are difficult to observe in winter, and especially with a Hodgkinson actinometer, which requires repeated "casts off" and hasty manipulation. Otherwise these mean results, so far as they go, *increase with the sun's progress.*

12. T. 5 presents abstracts of the observations already published of 1869 and 1879, and now expressed in terms of No. 2 (unit 0°·01 F.) and reduced to Z.D. 45°.

13. T. 6, T. 7, and T. 8 present abstracts of daily results respectively from T. 1, T. 2, and T. 3, *i.e.*, for Autumn 1880, Summer 1881, and Autumn 1881. [The results for the four autumns are exhibited graphically in Plate 1.]

14. With regard to T. 7, no reduction to Z.D. 45° has been attempted, for circumstances unfortunately did not permit of observing a long series *at the time.* In fact, observations in the summer were not contemplated, for, as a rule, that season is highly unfavourable for actinometrical work. As, however, the atmosphere unexpectedly became fine and clear, advantage was taken of the opportunity to do as much work as was practicable: for this purpose none but highly favourable days were admitted, and it is a point to be noticed that excepting on May 2, 6, and 9 *the visible conditions were decidedly good.* On the dates just mentioned a dry (dust?) haze visibly enveloped the observer, and the observations then taken are intended expressly to measure the effect of this haze. The results from T. 7 are

Mean from five clear days.	565
„ three hazy days.	535
Decrease.	30

or about $5\frac{1}{2}$ per cent. If this may happen at a height of 6,700 feet, and the haze was by no means of maximum density, what may be expected at lower and less favoured localities?

15. T. 8 exhibits an abstract of the bulk of the work now presented. The series extends over thirty-two days, in which four blanks occur unavoidably; the visible atmospheric conditions were excellent, and the work was done with great care and with two different kinds of instruments. Attention is called to the variable nature of the factor $\frac{\text{No. 2}}{\text{A}}$, due probably to imperfections in A.

16. Turning for a moment to another subject. As the photo-heliograph in *daily* work at Dehra Doon is under my charge, the negatives were available for finding areas of sun-spots in the autumn of 1881. My means for making these measurements in a limited time were not very rigorous; notwithstanding I believe the results obtained, of *visible* areas, are sufficiently exact. Nor was there leisure to find these areas for any *considerable* period, *i.e.*, before, after, and including that of the actinometrical observations of autumn 1881, and a considerable range of the kind is obviously essential for a complete inquiry as to connexion in time and circumstances between solar causes and terrestrial effects. Under existing pressure, I chose the daily negatives *corresponding* to the days of actinometer work, and obtained numerical values of areas for umbra and penumbra.

17. Looking now at curves C 2, the exhibit was intended to present curve projections of radiation and spot-areas *only in autumn* 1881; as, however, there is space on the exhibit, the autumn radiation curves for 1869, 1879, and 1880 have been added. The day-letters *for* 1881 have also been introduced, attention being here called to those containing a C or *c*. I repeat, that the presence of these letters by no means implies that the results are vitiated and rejectaneous from *visible* causes, but that merely *invisible* interpositions may possibly have prevailed. Now, as already said, the curves of 1881 are exhibits of *simultaneity* between radiation and spot-areas, and that being the argument, neither *contrariety* nor *similarity* decidedly appears between the two curves, which, therefore, in the exhibit, yield no answer as to spot temperature. If we discard the argument of simultaneity as improbable* in presence of our own heat arresting atmosphere alone,

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* I can only aver that my thermometer sunk in the ground 26 feet still continues to mark a *falling* temperature, although on the surface we are now fast approaching maximum summer heat (April 1, 1882).

then the question arises, for a given radiation effect how far *back* must we seek for the corresponding spot impulse (if any)? But my present view being necessarily limited to what appears on C 2, I advance (in imagination) the area curve by some three days, bringing, for instance, its features of 12th under that of radiation for 15th; when this is done, there is more general *resemblance* between the two curves, indicating increased radiation for increased spot-area. The middle or highest rising portions (16th to 29th) of spot-area will now correspond fairly with 18th (or 19th) to 1st of radiation; an interval about the same as a semi-solar rotation.

18. Looking at the radiation curve (autumn 1881) alone, it may be described as consisting of three hills (of which the last, alas, is cut short with the series and leads to nothing) and intervening valleys; between the apices of the first and second hills we have eleven days, between the valleys six and fourteen days.

19. This brings me to T. 9, where are tabulated the means of such of the autumnal results as happen to be forthcoming, lying between 15th October and 16th November for each year, and also the summer mean for 1881. Now, looking at the yearly autumn means reduced to 45° Z.D., it will be seen that they exhibit a continuous decrement of about 2 per cent. annually from 1879 to 1881. I think that they also have the appearance of truth about them, so far as they go, and, in fact in obtaining them considerable care and exactness was exercised, so that they may claim reliance. But when we look at the summer mean of 1881, which in my judgment is equally reliable, we find it only 565, at a solar zenith distance of but 15°·5, or nearly 30° *less* than the autumn Z.D. Unfortunately, I repeat, no long series could be taken in the summer, but the autumn long series in the same year (1881) gives a mean decrease of 47 radiation for an increase in Z.D. of 17° reckoned from Z.D. 46°; and accepting this as some approximate basis for reduction of the summer result from 15°·5 to 45°, I can only conjecture that this summer result, if thus reduced, would be *less* than the autumn result of the same by, say, at least, 40 to 50 units, or about half a degree F.* This represents so great and unexpected a change that one naturally looks for flaws in the summer observations, but I can find none. The mean is obtained from five days' observations taken carefully in (visibly speaking) unimpeachable weather, and the five results are accordant (see T. 7). Beyond this I can only say that I will certainly try to obtain other daily series, and a long series, in the approaching summer.

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* The solar negatives for the summer of 1881 are not with me now, but judging from silver prints in my possession, it is certain that sun-spots and faculæ were very sensibly absent in summer 1881 compared with sun-spot exhibits in the succeeding autumn; if this be accepted as cause, variation of radiation with sun-spot area would receive very decided confirmation in the present instance.

20. Next, I may first mention that by the "black bulb" thermometer is intended one of those unsatisfactory instruments, of which the bulb is blackened and enclosed in a fixed glass sphere some 3 inches in diameter, and that the same identical instrument was exposed and read on the same spot in 1879 to 1881. Now, according to what has been said, the actinometrical summer result, if true, indicates a sensible *decrease* of radiation compared with autumn, but the black bulb shows exactly the reverse, and reads 19° higher in the summer! There is also a similar contradiction between 1879 and 1880, where the black bulb gives a *rise* of 13 to an actinometrical *fall*. I confess my inability to account for these contradictions, which, however, I cannot regard as of serious importance, unless it be affirmed that black bulb thermometers are reliable: in this case there arises the question why the actinometer and the black bulb should be at such utter variance?

21. In concluding this paper, I venture to make a few remarks in respect to actinometrical observations. It is astonishing how considerably one's notion of the number of days suitable for actinometrical work undergoes modification after careful watching. I speak, of course, only of *visible* vitiating causes; of the invisible I know nought, if ought be known. And the presence of the former causes I now believe may be detected by a careful observer, at any place I am acquainted with, *on several days in the year:* no doubt one locality is far more suitable than another, but at the best a number of days will occur annually, when observations if made are worse than useless, for they are misleading. Hence in any project for continuous actinometrical work, the stations should be selected at least in pairs, so that the omissions at one of the two may be supplemented at the other.

22. Another point is this: suppose that at a given station the sun's meridional zenith distance at lowest is ζ, and at highest is Z, then the system of observation I suggest is as follows:—

(1.) Observe daily from 15 minutes before to 15 minutes after apparent local noon.

(2.) Observe daily a series of 6 sun and 5 shade observations when the sun on each side of the meridian is at zenith distance $=\zeta$ for the mean of the set.

(3.) Observe a series of 6 sun and 5 shade at every half hour from 9 A.M. to 3 P.M., if possible, on every day corresponding to a change of 3° declination between ζ and Z; thus if the sun's meridional zenith ranged in course of the year between zenith distances 7° and 54° (about), then by preference let the days of observation for long series be those days when the meridional zenith distances are successively 7°, 10°, and 13° . . . . 54° (about).

23. By means of (1), (2), (3) a complete connexion will be estab-

lished throughout the year's work at any *one* station, but as already remarked, I think a *pair* of stations essential. No doubt *several* pairs are really wanted, and sooner or later I believe this will come to be established *right round the globe,* for actinometer and solar photography and physics generally: at any rate this *must* happen, so soon as the leading nations of this earth take home to themselves the fact, that solar energy is *the* great force which utterly governs them, indirectly perhaps even in matters of commerce or war. Meanwhile, even one pair of actinometer stations will be welcome, and all the more so if established *at once.*

[It has not been thought necessary to print Tables I, II, III, which give the individual readings of the actinometers in full detail. The whole of the results have been collected by the author in Tables VI, VII, VIII. The remarks contained in the last column of Tables I, II, III have been extracted and placed after the corresponding Tables VI, VII, VIII, in which the results are collected.—G. G. S.]

Table IV.—Abstract of Long Series observed in 1879 and 1881.

1879.					1881.							
Apparent time.	Sun's zenith distance.	Change in zenith distance from noon.	Measured Radiation.		Apparent time.	Sun's zenith distance.	Change in zenith distance from noon.	Measured radiation.				
			A, glass off.*	In terms of No. 2, factor ·603. defect from noon.				Measured with No. 2.		Measured with A, glass off.	In terms of No. 2.	
								1881, 5th Nov.	Defect from noon.		1881, 5th Nov.	Defect from noon.
h. m.					h. m.							
8 0	75°·3	26°·9	765	115	9 0	63°·3	17°·1	525	39	872	526	30
9 0	64·9	16·5	881	45	9 30	58·6	12·4	538	26	901	543	13
10 0	56·3	7·9	924	19	10 0	54·5	8·3	549	15	918	554	2
11 0	50·5	2·1	946	6	10 30	51·0	4·8	558	6	930	561	−5
					11 0	48·4	2·2	560	4	937	565	−9
12 0	48·4		956		12 0	46·2		564		922	556	
					1 0	48·4	2·2	558	6	911	549	7
1 0	50·5	2·1	931	15	1 30	51·0	4·8	553	11	918	554	2
2 0	56·4	8·0	907	30	2 0	54·5	8·3	546	18	898	541	15
3 0	64·9	16·5	853	62	2 30	58·6	12·4	529	35	874	527	29
4 0	75·3	26·9	745	127	3 0	63·3	17·1	509	55	841	507	49

* Determined in 1879, see "Proc. Roy. Soc.," vol. 31, p. 191.

NOTE.—The Series of 1881 with A is unfortunately inconsistent, and is therefore rejected.

Table V.—Conversion of results 1869 and 1879 for A, glass off, to No. 2 or unit = 0°·01 Fah., and reduction to Z. D. 45°. (Abstract.)

Actinometer A.

1879.	Sun's meridional zenith distance.	A, glass off. Radiation, mean before and after noon.	A, glass off. Reduction to z. d. 45°.	A, glass off. Corrected radiation at z. d. 45°.	In terms of No. 2 at 45° z. d.
Oct. 31	44°·5	974	− 2	972	595
Nov. 1	44·8	946	1	945	579
2	45·1	978	+ 0	978	599
3	45·4	942	1	943	577
4	45·8	957	3	960	588
6	46·4	960	5	965	591
7	46·7	945	6	951	582
8	47·0	901	7	908	556
12	48·1	934	10	944	578
13	48·4	961	11	972	595
14	48·6	978	12	990	606
15	48·9	977	13	990	606
17	49·4	949	15	964	590
18	49·6	962	15	977	598
19	49·9	948	17	965	591
Mean	46·6				588

Excluding 17, 18, and 19.

1869.	Sun's meridional zenith distance.	A, glass off. Radiation, mean before and after noon.	A, glass off. Reduction to z. d. 45°.	A, glass off. Corrected radiation at z. d. 45°.	In terms of No. 2 at 45° z. d.
Oct. 27	43°·3	972	− 6	966	592
28	43·6	951	5	946	579
29	44·0	975	3	972	595
30	44·3	980	2	978	599
Nov. 3	45·6	1010	+ 2	1012	620
4	45·9	990	3	993	608
Mean	44·5				599

Table VI.—Abstract of daily results expressed in terms of No. 2 unit = 0°·01 Fah., and reduced to 45° Z. D.

	At Mussoorie, Autumn 1880.										
	Sun's meridional zenith distance.	Mean radiation for quarter-hour.		Mean of before and after noon.	At noon.				In terms of No. 2.		Day letter.
		Before noon.	After noon.		Barometer reduced to 32° F.	Thermometer.			At sun's z. d.	At z. d. 45°.	
						In shade.		In sun, black bulb.			
						Dry.	Wet.				
		Actinometer A.									
1880.											
October 18	40°·2	948±0·4		948	Not read.	64·4	58·3	125·5	581	571	A*a*
19	40·6	978±1·0	957±2·0	968		62·9	52·6	121·0	593	584	A*c*
20	41·0	939±0·8	927±1·3	933		61·9	53·1	120·0	571	563	A*a*
21	41·3	941±1·1	933±1·0	937		62·4	54·1	122·0	574	567	A*a*
26	43·1	964±0·9	960±1·3	962		62·6	50·9	120·0	589	585	A*a*
27	43·4	957±1·4	936±0·6	947		61·9	51·6	122·0	580	577	A*a*
Mean	41·6					62·7	53·4	122·1	581	575	

October 18.—No clouds near sun till noon, when they appeared about 45° from sun; stopped observing.
,, 19.—A few light clouds floating about but none near sun: throughout observations a slight haze over sun.
,, 20.—Fleecy clouds to E., N., and W., low down; none near sun.
,, 21.—Beautifully clear.
,, 26.—Clear.
,, 27.—No clouds within 45° of sun.

Table VII.—Abstract of daily results expressed in terms of No. 2 unit = 0°·01 Fah., but not reduced to Z. D. 45°.

	At Mussoorie, Summer, 1881.										
	Sun's meridional zenith distance.	Mean radiation for quarter-hour.		Mean of before and after noon.	At noon.				In terms of No. 2.		Day letter.
					Barometer reduced to 32° F.	Thermometer.					
						In shade.		In sun, black bulb.			
		Before noon.	After noon.			Dry.	Wet.		At sun's z. d.	At. z. d. 45°.	
1881.		Actinometer B (in terms of A).									
April 25	17°·2		941 ± 1 ·2	941	23 ·426	76 ·2	54·6	145 ·4	576		A*a*
26	16 ·9	913 ± 1 ·5	911 ± 2 ·5	912	·485	75 ·6	53 ·0	142 ·1	559		A*a*
May 2	15 ·0	855 ± 1 ·7	894 ± 1 ·5	875	·351	65 ·9	44·9	133 ·1	536		?
3	14 ·7	907 ± 1 ·3	909 ± 1 ·6	908	·375	63 ·4	45 ·5	135 ·8	556		A*b* (or *c*)
4	14 ·4	931 ± 0 ·6	935 ± 1 ·1	933	·391	69 ·4	49 ·0	139 ·4	571		A*b* (or *c*)
5	14 ·2	918 ± 1 ·0	916 ± 1 ·5	917	·452	73 ·5	52 ·3	139 ·8	562		A*b* (or *c*)
6	13 ·9	878 ± 1 ·3	861 ± 1 ·4	870	·505	74 ·8	55 ·0	138 ·0	533		?
9	13 ·1	882 ± 2 ·1	869 ± 1 ·4	876	·395	78 ·9	60 ·7	141 ·5	536		?
Mean	15 ·5	(Excluding 2nd, 6th and 9th.)			23 ·426	71 ·6	50 ·9	140 ·5	565		

April 25.—Beautifully clear.

May 3.—After the observations of 26th April, 1881, a dust haze set in, evidently an outcome of a storm in the plains below. It was evident that under these circumstances observations would be vitiated by the haze, so none were taken until to-day, when the set here given were made, expressly to show the effect of the haze. An hour or two after noon clouds formed, and it is worthy of note that as they formed the dust haze disappeared. No day letter can be assigned.

May 3.—Very slight dust haze, otherwise quite clear.

„ 4.—As yesterday.

„ 5.—As yesterday.

„ 6.—Day unfavourable. Dust haze sensible and rapidly increasing. No day letter can be assigned.

„ 9.—The 7th and 8th were very unfavourable. To-day the dust or dry haze is as on 6th May, 1881, and presents the usual conditions at this season. The observations here given were taken merely to show the effect of the haze. No day letter can be assigned.

Table VIII.—Abstract of daily results observed simultaneously with two Actinometers—Hodgkinson's A, and Stewart's No. 2, expressed in terms of No. 2 unit = 0°·01 F., and reduced to Z. D. 45°.

1881.	At Mussooree, Autumn 1881.																		
	Sun's meridional zenith distance.	Mean radiation for quarter-hour.				Mean of before and after noon.		At noon.				$\frac{\text{No. 2.}}{\text{A}}$	Reduction to Z.D. 45°.		Corrected radiation at 45°.				Day letter.
		Actinometer A.		Actinometer No. 2 (tube 1306).		Actr. A.	Actr. No. 2.	Barometer reduced to 32° F.	Thermometer.				Actr. A.	Actr. No. 2.	Actr. A.	Actr. No. 2.	A in terms of No. 2 factor = ·6124.	Mean of No. 2, and A in terms of No. 2.	
		Before noon.	After noon.	Before noon.	After noon.				In shade.		In sun, black bulb.								
									Dry.	Wet.									
Oct. 11	37°·6	949±1·3	936±1·1	566±0·6	568±0·8	943	567	23·343	63·9	57·5	124·8	0·6013	−27	−16	916	551	561	556	AC*a*
12	37·9	905±0·6	900±0·6	540±0·8	539±0·7	903	540	·318	65·1	57·4	124·9	·5980	25	15	878	525	538	532	AB*a*
14	38·7	936±1·0	934±0·7	572±1·8	568±1·1	935	570	·441	64·4	55·6	125·4	·6096	22	13	913	557	559	558	A*b*
15	39·1	938±1·1	937±1·3	570±0·2	579±1·7	938	575	·444	65·4	56·3	127·1	·6130	21	13	917	562	562	562	AB*b*
16	39·4	914±0·8	914±1·8	559±0·3	559±0·5	914	559	·405	64·0	56·5	128·5	·6116	18	11	896	548	549	549	AB*b*
17	39·8	903±0·8	896±1·3	558±0·8	557±0·4	900	558	·446	65·9	56·1	128·0	·6200	18	11	882	547	540	544	AC*b*
18	40·2	888±1·0	892±0·5	539±1·0	543±0·4	890	541	·477	64·9	57·5	126·5	·6079	17	10	873	531	535	533	AB (or C) *b*
19	40·5	899±2·2	905±1·6	553±0·8	557±0·6	902	555	·486	64·4	58·6	129·1	·6153	15	9	887	546	543	545	AC*b*
20	40·9	919±1·5	918±0·8	569±1·4	563±0·3	919	566	·504	62·9	58·5	123·2	·6159	13	8	906	558	555	557	AC*a*
22	41·6	955±0·6	951±0·9	590±0·4	593±1·5	953	592	·414	60·9	50·4	123·4	·6212	12	7	941	585	576	581	A*a*
24	42·3	954±0·9	957±0·5	596±0·4	598±0·5	956	597	·474	61·9	49·5	124·2	·6245	8	5	948	592	581	587	A*a*
25	42·6	951±0·7	944±0·7	588±0·9	589±0·7	948	589	·530	61·2	49·9	122·2	·6213	8	5	940	584	576	580	A*a*
26	43·0	971±0·8	971±0·7	602±0·3	604±0·5	971	603	·524	60·9	51·5	124·2	·6210	7	4	964	599	590	595	A*a*
28	43·7	944±0·9	944±0·6	586±0·6	585±0·4	944	585	·476	59·7	53·7	120·8	·6197	5	3	939	582	575	578	AB*a*
29	44·0	949±0·5	941±1·0			945		·521	58·4	53·0	119·5		3		942		577	577	A*b*
30	44·3	948±0·5	948±0·6	583±0·4	587±0·7	948	585	·467	59·4	54·0	120·0	·6171	2	1	946	584	579	582	A*b*
31	44·6	949±0·8	946±0·8	581±0·4	583±0·4	948	582	·437	59·2	53·0	120·2	·6139	2	1	946	581	579	580	A*b*
Nov. 1	45·0	929±1·1	917±0·5	548±0·4	547±0·4	923	548	·499	56·6	50·7	117·6	·5937	0	0	923	548	565	557	A*b*
2	45·3	921±0·3	924±0·7	563±0·4	564±0·4	923	564	·492	56·0	50·9	117·1	·6111	+ 1	+ 1	924	565	566	566	A*a*
3	45·6	938±0·6	944±0·4	571±0·5	569±0·4	941	570	·443	54·9	49·5	115·9	·6057	2	1	943	571	577	574	A*a*
4	45·9	923±0·4	915±0·8	559±0·6	557±0·4	919	558	·421	56·2	52·0	117·4	·6072	3	2	922	560	565	563	A*b*
5	46·2	925±0·8	919±0·9	563±0·5	564±0·4	922	564	·529	57·2	48·5	118·3	·6117	3	2	925	566	566	566	A*b*
6	46·5	921±0·8	931±1·1	570±0·5	570±0·5	926	570	·512	57·3	48·5	118·5	·6156	5	3	931	573	570	572	A*b*
7	46·8	925±0·7	917±1·3	564±0·6	564±0·4	921	564	·454	61·1	51·9	123·2	·6124	7	4	928	568	568	568	A*b*
8	47·1	930±0·7	930±0·6	562±0·3	570±0·7	930	566	·458	59·7	50·5	124·5	·6086	7	4	937	570	574	572	A*a*
9	47·4	902±0·4	901±0·8	554±0·5	554±0·4	902	554	·441	57·3	49·0	118·5	·6142	8	5	910	559	557	558	A*b*
10	47·7	926±0·4	924±0·2	564±0·4	568±0·4	925	566	·424	54·8	48·9	115·0	·6119	8	5	933	571	571	571	AC*c*
11	48·0	915±0·8	908±0·8	561±0·8	554±0·3	912	558	·462	55·9	49·3	119·0	·6118	10	6	922	564	565	565	AB*c*
an...	44·1	excluding	11th, 12th,	14th, and	15th Octob	er.		23·471	59·6	52·2	121·5					567	567		

October 11.—Wind in low puffs from S. Air and sky beautifully clear, except occasional small patches of c.c. which kept rising to W., and floating up to some 30° of sun, when they became invisible and could no longer be followed.

October 12.—As on 11th; except that the small patches of c.c. passed along a track some 8° to 20° *below* sun.

October 14.—A very favourable day. Light wind from S. Not even a speck of cloud visible; light haze towards plains S., and also slightly visible by dimming hills to N.

October 15.—A very favourable day and exactly like 14th, with one exception, *i.e.*, small light c.c. floated up from W. in a course passing some 15° to 25° below sun and disappeared when some 30° W. of sun. Wind S., at first very low, later on in somewhat fresh puffs.

October 16.—A very favourable day, except that c.c. (from very small up to 10° in diameter) kept passing at intervals in a track 10° to 20° below sun. Haze over plains more than yesterday, but no increase up here.

October 17.—A very still day, very clear, but small c.c. keep occasionally floating W. to E. in track about passing through sun; invisible near sun. Haze over plains rather increased since yesterday; haze is not visible overhead.

October 18.—Sky clear but for small c.c. floating about and disappearing 15° to 40° from sun; haze over plains increased. Hills some 30 miles to S. just visible; to N. view fine and clear. Wind S., in gusts but not strong. It is impossible to say how far, if at all, the clouds interfered.

October 19.—Sky beautifully blue and clear, but observation spoiled by a few patches of c.c. floating W. to E. across sun from time to time. Haze to S. over plains. A thunderstorm to N. last evening. Wind S. and low.

October 20.—A very fine day marred by not more than half-a-dozen c.c. floating about, some of which interposed. Sky otherwise blue and clear. All doubtful observations rejected. Wind in rather fresh gusts and southerly. Observations rejected when c.c. *visibly* interposed.

October 22.—A most brilliant day. Sky blue and without cloud or haze. Wind S. and rather fresh.

October 24.—Sky beautifully clear, except half-a-dozen small streaks of cir-strati about 10° above S. horizon, far from ⊙. Wind S. and sometimes in gusts, otherwise day highly favourable.

October 25.—Day beautifully fine. Wind S. and in gusts low and fresh.

October 26.—Day beautifully fine: two or three (only) small patches of c.s. moving W. to E. some 25° below sun. Wind S. and in rather strong gusts.

October 28.—To-day is peculiar. Strati and cumuli run along horizon for some 10° height from S. by E. and N. to S.E., besides a few small cirri to N. some 60° from sun; and in addition a few small c.c. kept floating up from W. to some 25° of sun (in track which would pass say 15° to 20° under sun) and then disappeared. Wind S. and in rather fresh gusts. No visible clouds approached the sun. Sky generally blue and bright.

October 29.—Sky deep blue, for about 10° below to 7° above sun. A little haze to S. over plains, c.c. along horizon from S.E. to N.W. A most perfect day for observations.

October 30.—Day beautifully fine. Not a speck of cloud except some cumuli on the snows (N) up to some 4° altitude only. Wind S. and occasionally in fresh gusts. Haze (smoke, vapour, and dust) over plains to S., but hills to N. quite clear. Weight for day 1·0.

October 31.—Day beautifully fine, as nearly as possible like yesterday (30th), except wind slightly stronger and haze over plain slightly increased.

November 1.—Day beautifully fine, like yesterday; wind as high, *i.e.*, in fresh gusts, from S. Clouds over snows, and one or two small patches floated a little way upward and disappeared some 60° or 80° from sun. Haze over plains increased slightly from smoke.

November 2.—Beautifully clear, not a cloud anywhere except a very few small ones over the snows.

November 3.—Beautifully clear day, perhaps the clearest we have had. Wind S. and in gusts of greater strength than usual.

November 4.—Day beautifully clear in respect to cloud, of which there is a narrow (1°) belt of c.s. some 8° above horizon to S. But as regards haze, this has been increasing (chiefly from smoke) over the plains, and to-day this haze is very slightly perceptible N.E. and W. as well.

November 5, 9.0.—Beautifully clear, not a speck of cloud, no wind. Over plains a good deal of haze (smoke), which is slightly visible against distant hills to N. (also E. and W.), so that we are in it.

9.30.—Just as at 9 A.M., except wind rising.

10.0.—As before, wind rising. Good series.

10.30.—Not a speck of cloud, as before.

11.0.—As before. Not a speck of cloud. Wind rising.

Noon.—A highly favourable day excepting the thin smoky haze in which we are imperceptibly enveloped—imperceptibly, *i.e.*, it does not appear between us and the sun, but it is quite visible over the plains (S.), and appears slightly against distant hills (15 to 50 miles) N.W. and E. Wind in low gusts.

1.0.—Fine, as before.

1.30.—Fine, as before.

2.0.—Fine, as before.

2.30.—Fine, as before. (Observer uncomfortably placed.)

3.0.—Fine, as before.

November 6.—Day beautifully clear, except slight (smoke) haze, which is as yesterday or perhaps a trifle less. Wind S. Observations good.

November 7.—Day beautifully clear. Smoke haze below to S., but hardly, if at all, visible against hills to N., slightly visible E. and W. Wind S. and low.

November 8.—Day beautifully clear now, but up to 8 or 9 A.M. fine strati were over southern sky. Wind S. and low.

November 9.—Day beautifully clear, except the smoke haze, which may be a trifle less than yesterday to S.E., S., and N.W.

November 10.—Day beautifully fine, and sky blue and clear some 50° around sun. Smoke haze more and we are slightly in it. A very few small patches of c.c. floating about, some perhaps in sun's track (E. to W.), others about 25° below sun. Wind S. and in rather strong gusts.

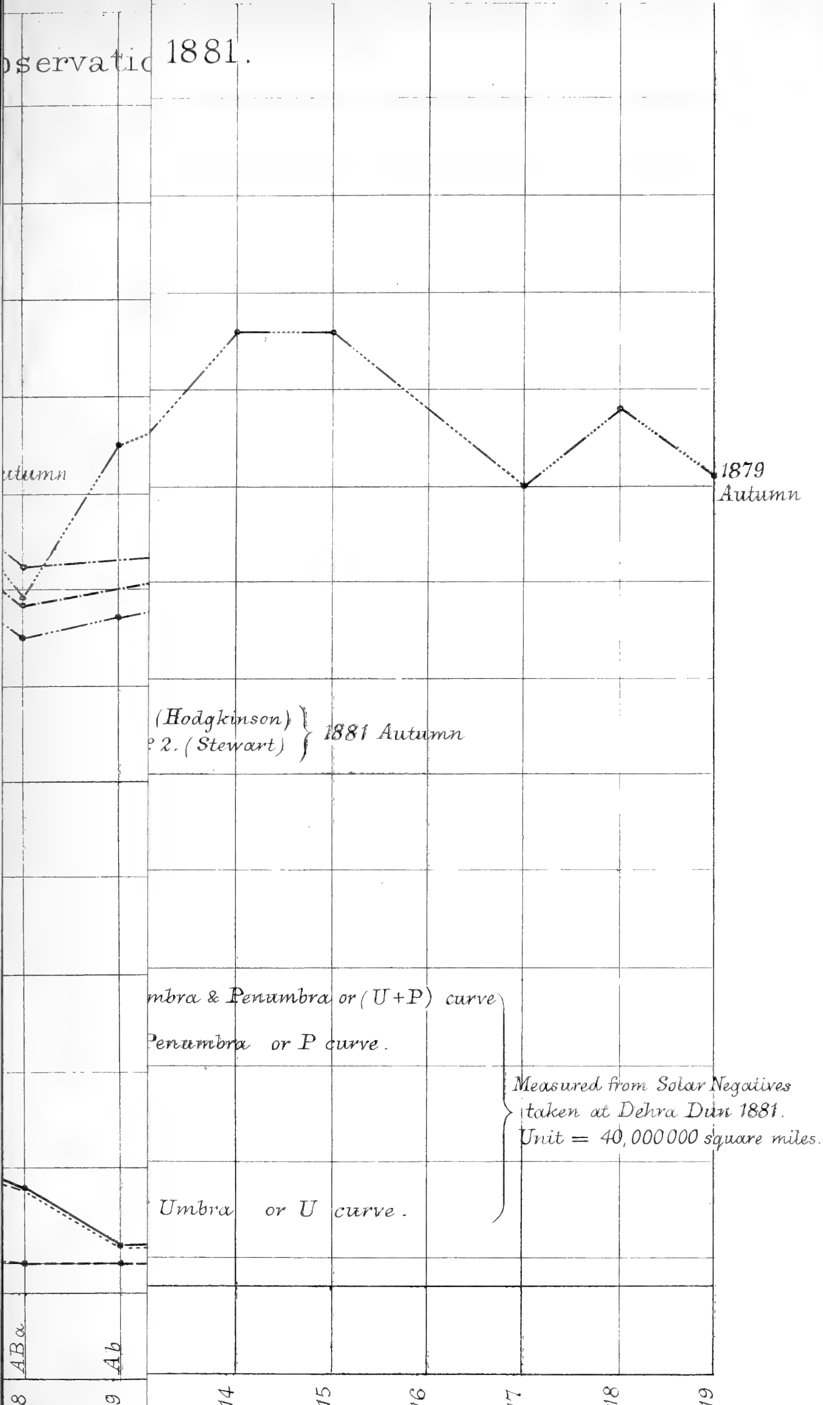
November 11.—Day beautifully fine; compared with yesterday there are more small patches of c.c. floating about, but not in tracks leading through sun; and the nearest disappearance of a patch was not under 25° from sun: there is also more smoke haze, and it is visible all round along horizon, so that we are in it, though it is quite invisible *above*. Wind S.E.

Table IX.—Mean autumnal actinometrical results between 15th October and 16th November for each year, and summer result for 1881, expressed in terms of Actinometer No. 2, and (excepting summer mean of 1881) reduced to 45° zenith distance.

		Sun's zenith distance.	Radiation in terms of No. 2.		Barometer.	Thermometer.			Observed with Actinometer.
			At sun's zenith distance.	Reduced to zenith dis. 45°.		In shade. Dry.	In shade. Wet.	In sun, black bulb.	
1869.	October 27 to November 4, for 6 days ..	44°·5	600	599	..	..	..	..	A.
1879.	,, 31 to ,, 15, for 12 ,, ..	46·6	584	588	23·457	55·5	46·5	108·8	,,
1880.	,, 18 to October 27, for 6 ,, ..	41·6	581	575	..	62·7	53·4	122·1	,,
1881.	April 25 to May 9, for 5 ,, ..	15·5	565	..	23·426	71·6	50·9	140·5	B.
,,	October 16 to November 11, for 23 ,, ..	44·1	569	567*	23·471	59·6	52·2	121·5	No. 2.
,,	,, to ,, for 24 ,, ..	44·1	569	567*					A.

Note.—In 1869 the daily series of actinometrical observations ranged for about half an hour before and after noon. In 1879 for exactly (very nearly) 30 minutes before and after noon, and for 1880 and 1881 the range was reduced to 15 minutes before and after noon. As regards barometer and thermometer readings none were taken (methodically) in 1869: in 1879 and 1881 they were taken regularly at noon, *i.e.*, middle of actinometrical observations, but in 1880 the meteorological instruments were read shortly after 0h. 30m., excepting the barometer, which was not read.

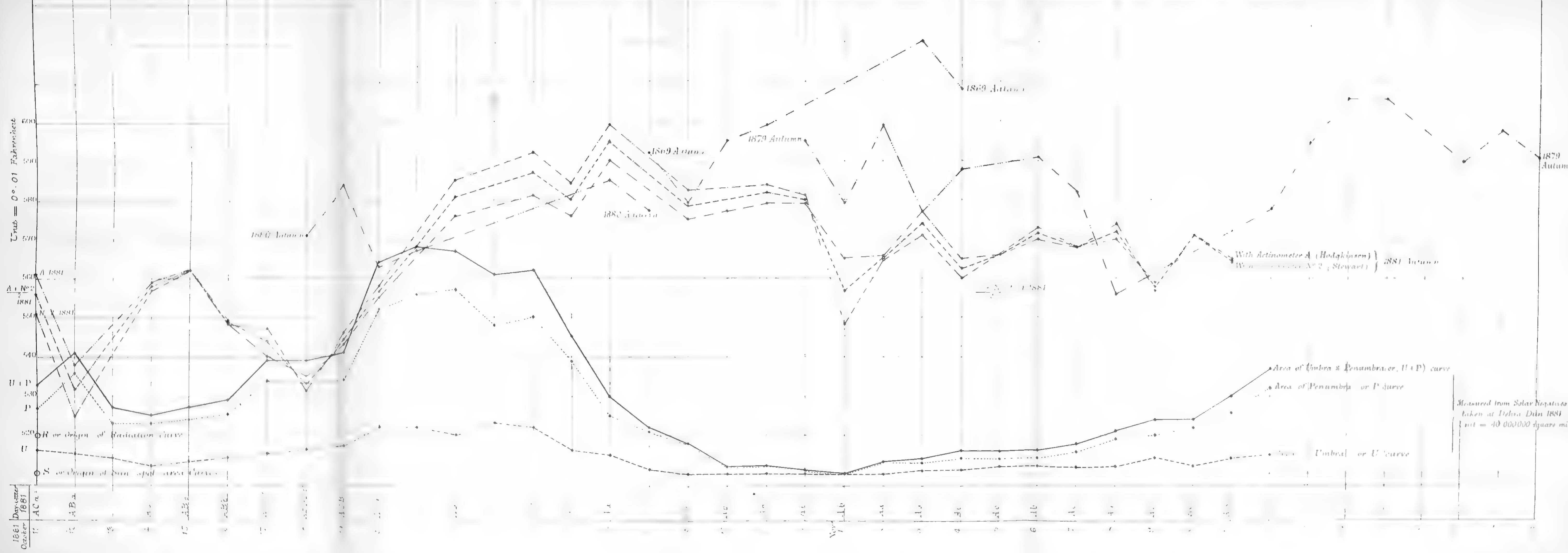
[*] These two results necessarily agree, because in converting A into No. 2 the mean ratio or factor used was $\frac{\text{No. 2}}{\text{A.}}$.



West Newman & Co lith.

Curves C 2

Daily Solar Radiation Curves from Actinometrical Observations in Autumns of 1869, '79, '80 & '81 with corresponding Sun spot curves in 1881.



V. "On the Cause of the Light Border frequently noticed in Photographs just outside the Outline of a Dark Body seen against the Sky; with some Introductory Remarks on Phosphorescence." By Professor G. G. STOKES, Sec. R.S. Received May 20, 1882.

An observation I made the other day with solar phosphori, though not involving anything new in principle, suggested to me an explanation of the above phenomenon which seems to me very likely to be the true one. On inquiring from Captain Abney whether it had already been explained, he wrote: "The usual explanation of the phenomenon you describe is that the silver solution on the part of the plate on which the dark objects fall has nowhere to deposit, and hence the metallic silver is deposited to the nearest part strongly acted upon by light." As this explanation seems to me to involve some difficulties, I venture to offer another.

1. I will first mention the suggestive experiment, which is not wholly uninteresting on its own account, as affording a pretty illustration of what is already known, and furnishing an easy and rapid mode of determining in a rough way the character of the absorption of media for rays of low refrangibility.

The sun's light is reflected horizontally into a darkened room, and passed through a lens,* of considerable aperture for its focal length. A phosphorus is taken, suppose sulphide of calcium giving out a deep blue light,† and a position chosen for it which may be varied at pleasure, but which I will suppose to be nearer to the lens than its principal focus, at a place where a section of the pencil passing through the lens by a plane perpendicular to its axis shows the caustic surface well developed. A screen is then placed to intercept the pencil passing through the lens, and the phosphorus is exposed to sunlight or diffuse daylight, so as to be uniformly luminous, and is then placed in position; the screen is then removed for a very short time and then replaced, and the effect on the phosphorus is observed.

Under the circumstances described there is seen a circular disk of blue light, much brighter than the general ground, where the excitement of the phosphorus has been refreshed. This is separated by a dark halo from the general ground, which shines by virtue of the original excitement, not having been touched by the rays which came through the lens.

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* The lens actually used was one of crown-glass which I happened to have; a lens of flint glass would have been better, as giving more separation of the caustic surfaces for the different colours.

† The experiments were actually made, partly with a tablet painted with Balmain's luminous paint, partly with sulphide of calcium and other phosphori in powder.

2. The halo is due to the action of the less refrangible rays, which, as is well known, discharge the phosphorescence. Their first effect, as is also known, is however to cause the phosphorus to give out light; and if the exposure were very brief, or else the intensity of the discharging rays were sufficiently reduced, the part where they acted was seen to glow with a greenish light, which faded much more rapidly than the deep blue, so that after a short time it became relatively dark.

3. This change of colour of the phosphorescent light can hardly fail to have been noticed, but I have not seen mention of it. In this respect the effect of the admission of the discharging rays is quite different from that of warming the phosphorus, which as is known causes the phosphorus to be brighter for a time, and then to cease phosphorescing till it is excited afresh. The difference is one which it seems important to bear in mind in relation to theory. Warming the phosphorus seems to set the molecules more free to execute vibrations of the same character as those produced by the action of the rays of high refrangibility. But the action of the discharging rays changes the character of the molecular vibrations, converting them into others having on the whole a lower refrangibility, and being much less lasting.

4. Accordingly when the phosphorus is acted on simultaneously by light containing rays of various refrangibilities, the tint of the resulting phosphorescence, and its more or less lasting character, depend materially on the proportion between the exciting and discharging rays emanating from the source of light. Thus daylight gives a bluer and more lasting phosphorescence than gaslight or lamplight. I took a tablet which had been exposed to the evening light, and had got rather faint, and, covering half of it with a book, I exposed the other half to gaslight. On carrying it into the dark, the freshly exposed half was seen to be much the brighter, the light being, however, whitish, but after some considerable time the unexposed half was the brighter of the two.

Again, on exposing a tablet, in one part covered with a glass vessel containing a solution of ammonio-sulphate of copper, to the radiation from a gas flame, the covered part was seen to be decidedly bluer than the rest, the phosphorescence of which was whitish. The former part, usually brighter at first than the rest, was sure to be so after a very little time. The reason of this is plain after what precedes.

A solution of chromate of potash is particularly well suited for a ray filter when the object is to discharge the phosphorescence of sulphide of calcium. While it stops the exciting rays it is transparent for nearly the whole of the discharging rays. The phosphorescence is accordingly a good deal more quickly discharged under such a solution than under red glass, which along with the exciting rays absorbs also a much larger proportion than the chromate of the discharging rays.

5. I will mention only one instance of the application of this arrangement to the study of absorption. On placing before excited sulphide of calcium a plate of ebonite given me by Mr. Preece as a specimen of the transparent kind for certain rays of low refrangibility, and then removing the intercepting screen from the lens, the transmission of a radiation through the ebonite was immediately shown by the production of the greenish light above-mentioned. Of course, after a sufficient time the part acted on became dark.

6. I will mention two more observations as leading on to the explanation of the photographic phenomenon which I have to suggest.

In a dark room, an image of the flame of a paraffin lamp was thrown by a lens on to a phosphorescent tablet. On intercepting the incident rays after no great exposure of the tablet, the place of the image was naturally seen to be luminous, with a bluish light. On forming in a similar manner an image of an aperture in the window shutter, illuminated by the light of an overcast sky reflected horizontally by a looking-glass outside, this image of course was luminous; it was brighter than the other. On now allowing both lights to act simultaneously on the tablet, the image of the flame being arranged to fall in the middle of the larger image of the aperture, and after a suitable exposure cutting off both lights simultaneously, the place of the image of the aperture on which the image of the lamp had fallen was seen to be *less* luminous than the remainder, which had been excited by daylight alone. The reason is plain. The proportion of rays of lower to rays of higher refrangibility is much greater in lamplight than in the light of the sky; so that the addition of the lamplight did more harm by the action of the discharging rays which it contained on the phosphorescence produced by the daylight, than it could do good by its own contribution to the phosphorescence.

7. The other observation was as follows:—The same tablet was laid horizontally on a lawn on a bright day towards evening, when the sun was moderately low, and a pole was stuck in the grass in front of it, so as to cast a shadow on the tablet. After a brief exposure the tablet was covered with a dark cloth, and carried into a dark room for examination.

It was found that the place of the *shadow* was *brighter* than the general ground, and also a deeper blue. For a short distance on both sides of the shadow the phosphorescence was a little feebler than at a greater distance.

This shows that, though the direct rays of the sun by themselves alone would have strongly excited the phosphorus, yet acting along with the diffused light from all parts of the sky, they did more harm than good. They behaved, in fact, like the rays from the lamp in the experiment of § 6. The slightly inferior luminosity of the parts to some little distance on both sides of that on which the shadow fell, shows that the loss of the diffuse light corresponding to the portion of the sky cut off by the pole was quite sensible when that portion lay very near the sun.

All this falls in very well with what we know of the nature of the direct sunlight and the light from the sky. In passing through the atmosphere, the direct rays of the sun get obstructed by very minute particles of dust, globules of water forming a haze too tenuous to be noticed, &c. The veil is virtually coarser for blue than for red light, so that in the unimpeded light the proportion of the rays of low to those of high refrangibility goes on continually increasing, the effect by the time the rays reach the earth increasing as the sun gets lower, and has accordingly a greater stretch of air to get through. Of the light falling upon the obstructing particles, a portion might be absorbed in the case of particles of very opaque substances, but usually there would be little loss this way, and the greater part would be diffused by reflection and diffraction. This diffused light, in which there is a predominance of the rays of higher refrangibility, would naturally be strongest in directions not very far from that of the direct light; and the loss accordingly of a portion of it where it is strongest, in consequence of interception by the pole in front of the tablet, accounts for the fact that the borders of the place of the shadow were seen to be a little less luminous than the parts at a distance.

8. The observations on phosphorescence just described have now prepared the way for the explanation I have to suggest of the photographic phenomenon.

It is known that, with certain preparations, if a plate be exposed for a very short time to diffuse daylight, and be then exposed to a pure spectrum in a dark room, on subsequently developing the image it is found that while the more refrangible rays have acted positively, that is, in the manner of light in general, a certain portion of the less refrangible have acted in an opposite way, having undone the action of the diffuse daylight to which the plate was exposed in the first instance.

It appears then that in photography, as in phosphorescence, there may in certain cases be an antagonistic action between the more and less refrangible rays, so that it stands to reason that the withdrawal of the latter might promote the effect of the former.

Now the objective of a photographic camera is ordinarily chemically corrected; that is to say, the minimum focal length is made to lie, not in the brightest part of the spectrum, as in a telescope, but in the part which has strongest chemical action. What this is, depends more or less on the particular substance acted on; but taking the preparations most usually employed, it may be said to lie about the indigo or violet. Such an objective would be much under-corrected for the red, which accordingly would be much out of focus, and the ultra-red still more so.

When such a camera is directed to a uniform bright object, such as a portion of overcast sky, the proportion of the rays of different refrangibilities to one another is just the same as if all the colours were in focus together; but it is otherwise near the edge of a dark object on a light ground. As regards the rays in focus, there is a sharp transition from light to dark; but as regards rays out of focus, the transition from light to dark though rapid is continuous. It is, of course, more nearly abrupt the more nearly the rays are in focus. Just at the outline of the object there would be half illumination as regards the rays out of focus. On receding from the outline on the bright side, the illumination would go on increasing, until on getting to a distance equal to the radius of the circle of diffusion (from being out of focus) of the particular colour under consideration the full intensity would be reached. Suppose, now, that on the sensitive plate the rays of low refrangibility tend to oppose the action of those of high refrangibility, or say act negatively, then just outside the outline the active rays, being sharply in focus, are in full force, but the negative rays have not yet acquired their full intensity. At an equal distance from the outline on the dark side the positive rays are absent, and the negative rays have nothing to oppose, and therefore simply do nothing.

9. I am well aware that this explanation has need of being confronted with experiment. But not being myself used to photographic manipulation, I was unwilling to spend time in attempting to do what could so much better be done by others. I will, therefore, merely indicate briefly what the theory would lead us to expect.

We might expect, therefore, that the formation of the fringe of extra brightness would depend:—

(1.) Very materially upon the chemical preparation employed. Those which most strongly exhibit the negative effect on exposure to a spectrum after a brief exposure to diffuse light might be expected to show it the most strongly.

(2.) Upon the character of the light. If the light of the bright ground be somewhat yellowish, indicating a deficiency in the more refrangible rays, the antagonistic effect would seem likely to be more strongly developed, and, therefore, the phenomenon might be expected to be more pronounced.

(3.) To a certain extent on the correction of the objective of the camera. An objective which was strictly chemically corrected might be expected to show the effect better than one in which the chemical and optical foci were made to coincide, and much better than one which was corrected for the visual rays.

It is needless to say that on any theory the light must not be too bright or the exposure too long; for we cannot have the exhibition (in the positive) of a brighter border to a ground which is white already.

P.S.—Before presenting the above paper to the Royal Society I submitted it to Captain Abney, as one of the highest authorities in scientific photography, asking whether he knew of anything to disprove the suggested explanation. He replied that he thought the explanation a possible one, encouraged me to present the paper, and kindly expressed the intention of submitting the question to the test of experiment.

I have referred to the photographic action of the more and less refrangible rays as antagonistic. This is practically true so far as the explanation I have ventured to offer is concerned, inasmuch as the more refrangible rays convert a salt of silver which is not developed into one which is developable, while the less refrangible convert the latter into one which is not developable. But Captain Abney has pointed out to me that though the first and third salts cannot be distinguished by appearance, nor by the action of the developing solution, they are nevertheless not the same, so that the two actions of the rays are not, rigorously speaking, antagonistic, inasmuch as the one is not strictly the reverse of the other. Thus with bromide of silver the explanation of the observed phenomena, according to Captain Abney, is that the undevelopable bromide is converted, chiefly by the action of the more refrangible rays, into a subbromide, which is developable; and this again is converted, chiefly by the action of the less refrangible rays, into an oxybromide, which is undevelopable. As however under the ordinary circumstances for obtaining a good picture the action of the light is chiefly of the first kind, and a much longer exposure would be required to bring out prominently the second kind of action, the explanation I have suggested is not virtually affected, though the two actions could not be prolonged indefinitely, as in the illustrative experiment in phosphorescence described in § 6.

June 10.

The Society adjourned over the Whitsuntide Recess to Thursday, June 15th.

June 8, 1882.

The Annual Meeting for the Election of Fellows was held this day.

THE PRESIDENT in the Chair.

The Statutes relating to the election of Fellows having been read, Professor François de Chaumont and the Rev. R. Harley were, with the consent of the Society, nominated Scrutators to assist the Secretaries in examining the lists.

The votes of the Fellows present were then collected, and the following candidates were declared duly elected into the Society.

Ball, Professor Valentine, M.A.
Brady, George Stewardson, M.D., F.L.S.
Buchanan, George, M.D.
Clarke, Charles Baron, M.A., F.L.S.
Darwin, Francis, M.A., F.L.S.
Dittmar, Professor William, F.C.S.
Gaskell, Walter Holbrook, M.D.
Glazebrook, Richard Tetley, M.A.
Godman, Frederic Du Cane, F.L.S.
Hutchinson, Professor Jonathan, F.R.C.S.
Liversidge, Professor Archibald, F.G.S.
Malet, Professor John C., M.A.
Niven, William Davidson, M.A.
Palgrave, Robert Henry Inglis, F.S.S.
Weldon, Walter, F.C.S.

Thanks were given to the Scrutators.

Presents, April 20, 1882.

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June 15, 1882.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

Mr. Gabriel Auguste Daubrée (Foreign Member), Dr. George Stewardson Brady, Dr. George Buchanan, Mr. Francis Darwin, Professor William Dittmar, Dr. Walter Holbrook Gaskell, Mr. William Davidson Niven, and Mr. Robert Henry Inglis Palgrave were admitted into the Society.

The President read a despatch from H.M. Consul-General at Florence, transmitted through the Foreign Office, giving an account of a commemoration in honour of the late Charles Darwin, held in the great hall of the Istituto di Studi Superiori.

A preliminary (oral) Statement of Results of observation of the total Eclipse of the Sun on May 17, as seen in Egypt, was made by Mr. J. N. Lockyer, F.R.S.

The following Papers were read:—

I. "Researches on Spectrum Photography in relation to New Methods of Quantitative Chemical Analysis." By W. N. Hartley, F.R.S.E., &c., Professor of Chemistry, Royal College of Science, Dublin. Communicated by Professor G. G. Stokes, Sec. R.S. Received May 19, 1882.

Preliminary Note.

(1.) Since I perfected the instrument employed by me in investigating the molecular structure of organic compounds, I have been engaged in studying the spark spectra of elementary bodies with the object of applying my method of working to the purposes of technical chemical analysis. Progress in the course of this research has proved it to be necessary to investigate all kinds of spectra *de novo* in the hitherto little explored *ultra-violet* region.

Some fourteen years' practice in photography has convinced me that when a plate is properly exposed the development of the image is the simplest of all operations; in order, therefore, to simplify spectroscopic work, I have carefully ascertained the time of exposure required to produce the most characteristic spectra under various conditions, such as intensity of spark and conductivity, &c., of the electrodes. This, in the instrument I prefer to use, is generally a period of half a minute. ("Journal of the Chem. Soc.," vol. xli, p. 84, 1882.)

(2.) A long series of experiments has been made with the object of comparing the spectra of various compounds in solution with those of the elements they contain. In the process of photographing the spectra of solutions it is desirable to eliminate all foreign lines as far as possible, hence the selection of suitable electrodes was a matter of the first consideration, the method of working being almost entirely dependent on this for its accuracy and value. No method like that of Bunsen is convenient, in which charcoal points are employed in conjunction with a spark from a coil without a condenser, by reason of the prolonged exposure rendered necessary, the intensity of the emitted rays being small. Electrodes of gold, platinum, iridium, and other metals were used, and those of gold proved decidedly the best, as containing the fewest lines and the metal being a most excellent conductor of electricity.

All these metals are, however, useless compared with electrodes of graphite. The spectrum of graphite consists of eleven or twelve insignificant lines due to the carbon, and about sixty-six lines and bands due to air.

The air-lines are easily recognised from their "physiognomie," as M. Lecoq de Boisbaudran calls it, or as I have elsewhere described this peculiarity in relation to spectrum photographs, their "*graphic character*." In no case with the intensity of spark which I employ and the normal exposure have I ever been troubled with the presence of such impurities as may be contained in points of good Siberian or Ceylon graphite. Such points have been submitted to the continuous action of a condensed spark for something like ten hours at a time, the same solution being used and the electrodes unaltered. It is usual to take fresh electrodes for each solution.

(3.) In comparing the spectra of solutions of salts with those of metallic electrodes, it was found that in almost all cases the lines of metals were exactly reproduced from the solution, the *graphic character* being retained except in regard to their continuity. Discontinuous but long lines, or in certain cases even short lines, appear as long lines in the spectra taken from solutions. The peculiarities of the spectra of magnesium, of cadmium, and iron, were exactly reproduced, line for line, from the chlorides. An alteration was noticed in the spectrum of graphite, the short lines became long, that is to say, discontinuous became continuous lines, when the electrodes were wetted with water or acids.

An exceptional instance of variation in a spectrum was seen in that of zinc. The pure metal exhibits a series of highly characteristic excessively short lines or dots, which are totally absent from the photographs of solutions of zinc made from the same metal. Certain discontinuous lines in the spectrum of iridium become continuous when moistened with calcic chloride solution. It has been remarked by me elsewhere (*loc. cit.*) that the more volatile, and I may now add, the more unoxidisable a metal, the more continuous are its lines. The compounds in solution are more volatile than the metals, and hence the greater continuity in the lines. In the case of graphite it is doubtless a volatile carbon compound, either carbon dioxide or a hydrocarbon, which is formed by the heat of the spark when the points are moistened with water. In the case of iridium it is difficult to suppose that the calcic chloride solution forms a chloride by the simple action of heat on such a refractory metal; but this is the only explanation that will account for the greater continuity of the lines. Insoluble compounds give no spectra when mixed with water or glycerine and exposed to the spark. The non-metallic constituents of salts do not yield any marked series of lines, and therefore do not obscure the metallic spectra.

The spectrum of aluminium as obtained from perfectly pure solutions is free from a group of short or discontinuous lines seen in my published photographs of spectra. By prolonged exposure, as I have elsewhere shown, these lines have been proved to be due to iron. The spectrum of aluminium is thus proved to be a very simple one. In all these spectra the rays lying between 4500 and 2000 on the scale of wave-lengths are completely focussed on one plate, and the relative intensities of the lines exhibit the relative intensities of the rays. Any modification in the relative intensity of a line or in its length is accurately registered on the sensitive plate. As many as fifteen different spectra have been photographed on one plate, and developed by one immersion in the developing solution. It has been proved experimentally that *accidental alterations* in the period of normal exposure, which are not very noticeable, do not affect the spectra. Any irregularities such as may be unavoidable in the passage of the spark do not alter the normal densities of the images of the various rays. The development of the photographs is completed in about thirty seconds. These points are of vital importance in placing this method of working on such a basis that it may be employed in quantitative methods of chemical analysis, for if the intensity of the rays be so great that the period of exposure is rendered much shorter, difficulties would arise in obtaining photographs with neither more nor less than the requisite density. And, again, were the exposure much prolonged the method would become somewhat tedious, or, at least, it would be impaired in value.

(4.) Of all methods likely to yield results of practical importance in estimating the relative proportions of the constituents of either an alloy or a mineral, only those have recommended themselves to me which depend upon the use of solutions; and for the reason that most alloys are not homogeneous, and the portion of a metallic electrode exposed to the action of the spark is volatilised from one point, and is too minute in quantity to represent the composition of the mass. Now, the composition of a solution represents in every part the composition of the entire mass dissolved; it is, therefore, quite unimportant how small a fraction of it is used for the purpose of obtaining the spectrum of its constituents.

It is a remarkable fact that at the present time we know little or nothing of the sensitiveness of the spectrum reaction *under various conditions*, notwithstanding that such knowledge is absolutely necessary for the purpose of giving stability to numerous theories and arguments which are based on spectrum observations. I have made some experiments in this direction by determining the extent of dilution which serves to modify in various ways the spectra of solutions of metallic salts, and that which finally causes the extinction of the most persistent line or lines. The sensitiveness of the reaction varies with different elements and with the period of exposure, the intensity of the spark, and other conditions; I have no difficulty whatever, when working in the manner here indicated, in recognising spectra yielded by solutions which contain no more than $\frac{1}{1000}$th of a *per cent.* of calcium, silver, copper, and $\frac{1}{10000}$th of a *per cent.* of manganese. It is necessary, however, for me to withhold a full account of my experiments until I have determined the wave-lengths of the lines in the various spectra under observation, for it is quite impossible to describe the changes in the spectra without reference to accurate measurements of the metallic lines. For some time past Mr. W. E. Adeney has been working in conjunction with me at these determinations, and I hope with as little delay as possible to have the honour of submitting to the Royal Society all details here omitted, both with regard to these new methods of analysis, and the wave-length determinations.

II. "On the Reversal of the Metallic Lines as seen in Over-exposed Photographs of Spectra." By W. N. Hartley, F.R.S.E., &c., Professor of Chemistry, Royal College of Science, Dublin. Communicated by Prof. G. G. Stokes, Sec. R.S. Received May 19, 1882.

In preparing series of photographs of metallic elements when their spectra are obtained by the action of a condensed spark passed between metallic electrodes, I have been very careful to ascertain the exact period of exposure of the sensitive plate to the rays, which will bring out the most characteristic lines without the additional diffused rays of the air-spectrum; at the same time very delicate and feeble air-lines are adequately shown. This has always been accomplished by making a series of *comparative exposures.* With gelatine emulsion dry plates great latitude in exposure is capable of yielding perfectly satisfactory photographs. An under-exposed plate is not easy to develop, in order that the usual density for the strong lines as seen in a good negative may be gained. The air-lines are generally very feeble or altogether omitted. An over-exposed plate is likewise difficult to develop; it yields a thin flat image, and more or less marked indications of a continuous air-spectrum are seen.

Over-exposure, even when not excessive, is liable to cause strong lines to appear reversed. I have mentioned in my paper "Notes on Certain Photographs of the Ultra-Violet Spectra of Elementary Bodies" ("Journal of the Chemical Society," vol. xli, p. 89, 1882), that sometimes lines appear reversed in one photograph, but not in another. This did not seem at all likely, or even possible, to be caused by over-exposure, because the two periods differed only by a minute; but I have small doubts now on the matter. The conversion of what is called a negative into a positive image by excessive exposure has been already noticed by Mr. C. Bennett ("British Journal of Photography," 1878), by Captain Abney, who investigated the nature of the change ("Phil. Mag." [5] 10, p. 200), occurring in the sensitive film, and by M. Janssen ("Comptes Rendus," 90, pp. 1447—1448).

In illustration of this phenomenon, I may mention a remarkable result I obtained on one occasion when photographing a landscape. I endeavoured to secure a picture with detail in a shaded foreground, and a direct view of the setting sun, with mountains in the middle distance, and strongly illuminated as well as dark clouds. In one case I succeeded remarkably well, but in another plate the foreground was good, but the sun was completely reversed. The negative image was clear glass and the sun printed black. What should have been a negative in the strong lights became a positive. Again, by exposing a plate to the cadmium spectrum, the whole of the metallic lines were rendered distinctly, but with a flatness and want of density, the whole of the strong air-lines at the least refrangible end of the spectrum were, however, completely reversed.

Any strong lines may be reversed by over-exposure without materially altering the appearance of the rest of the spectrum. This is particularly the case with the lines of the metals magnesium, aluminium, and indium, but particularly so with magnesium. The reversal takes place in the centre of the line, that is to say, where the radiation is most active. Except by the method of comparative exposures, which I have always employed, it would be impossible to say whether a reversal was due to an absorbed ray or an over-exposed plate.

M. Cornu has shown that the quadruple group of rays in the magnesium spectrum may become quintuple or sextuple, according to the increased intensity of the spark employed. This is precisely what might happen if one reversal by over-exposure were followed by a second. Such reversals might be looked for if under the conditions of the stronger spark the exposure of the plate were not shortened, because the first and third of the four lines are stronger than the other two, and they would therefore be the first and second to suffer reversal. The reversal would split the lines in two, and hence produce the appearance of a sextuple group. In order to ascertain whether this might readily occur in the magnesium spectrum, some observations were made with plates containing several photographs obtained by different periods of exposure. Thus the first spectrum was the result of ten seconds, the second of half a minute, and others various times extending to half an hour. The quadruple group was not affected in the way observed by M. Cornu, from which fact it would appear that the division of the lines was caused by a reversal which was the result of absorption of the central portion of the ray or rays. In the two photographs obtained by the longest exposures, especially in the last, the triplet *b'* between K and L became a quadruple group by reason of the most refrangible line being split into two by a reversal, the cause of which was nothing more than over-exposure. In the quadruple group previously mentioned the lines were totally reversed or not at all. This subject of reversal by over-exposure is one well deserving the attention of those who are engaged in the study of solar physics. Comparative exposures should be methodically employed to confirm the accuracy of observations made entirely by the aid of photographic representations of spectra. Especially is this desirable when gelatine or other dry plates containing organic matter are in use.

III. "Experiments on the Value of the Ohm." Part I. By R. T. GLAZEBROOK, M.A., Fellow and Assistant Lecturer of Trinity College, Demonstrator at the Cavendish Laboratory, Cambridge, and J. M. DODDS, B.A., Fellow of St. Peter's College. Part II. By R. T. GLAZEBROOK, and E. B. SARGANT, M.A., Trinity College. Communicated by LORD RAYLEIGH, F.R.S. Received May 24, 1882.

(Abstract.)

The method of the experiments is a modification of those of Kirchhoff and Rowland.

Two coils of copper wire of about 25 centims. radius, each containing

about 780 turns, were placed with their mean planes parallel and at a known distance apart. The coefficient of mutual induction between the two can be found from the geometrical data; let this be M. Let one of the coils be connected in circuit with a ballistic galvanometer, and let R be the resistance in centimetres per second of the circuit. Let a steady current of intensity i be circulating in the other coil—the primary. On reversing this current an induction current, of which the amount is $\frac{2Mi}{R}$, is produced in the secondary circuit, and the galvanometer needle is disturbed from rest; if β be the first throw of the needle, T the time of a complete vibration, λ the coefficient of damping, τ that of torsion, G the galvanometer constant, and H the horizontal intensity of the earth's magnetism, we have

$$\frac{2Mi}{R}=\frac{H(1+\tau)}{G}\frac{T}{\pi}\cdot\left(1+\frac{\tau}{2}\right)\left(1+\frac{\lambda}{2}\right)\sin\frac{\beta}{2}.$$

The galvanometer was then connected in series with a large resistance coil, in our case of about 3,000 ohms; let S be the resistance of the galvanometer and this coil. The two extremities of the resistance S were connected with two points in the primary circuit, the resistance between which was about 1 ohm; let this resistance be V. Then of the primary current i, an amount $\frac{V}{S+V}i$, is transmitted through the galvanometer, and if α be the deflection of the needle, we have

$$\frac{V}{S+V}i=\frac{H(1+\tau)}{G}\tan\alpha.$$

Eliminating i, G and H, we obtain

$$R=\frac{2\pi M}{T\left(1+\frac{\tau}{2}\right)\left(1+\frac{\lambda}{2}\right)}\cdot\frac{S+V}{V}\cdot\frac{\tan\alpha}{\sin\frac{\beta}{2}}.$$

And if $\bar{R}$ be the value of R in ohms, the ratio $\frac{R}{\bar{R}}$ gives us the value of the ohm in centimetres per second.

The coils and galvanometer were wound for this purpose with great care by Professor Chrystal under the supervision of the late Professor Clerk Maxwell. Professor Chrystal's removal from Cambridge prevented the completion of the experiments by him.

For a detailed account of the precautions necessary, the methods of making the observations, and the comparison of the resistance coils used, reference must be made to the paper.

Each experiment involves eight observations of throw due to the induction current and two of deflection; the values of the deflection being obtained from observations of the oscillations of the needle about its position of rest, the chance of error is much smaller than in the throws.

Each set of experiments is the mean of four; one for each of the four positions in which the coils could be placed by inverting first one and then the other without altering the distance between their centres.

Part I, which we regard as preliminary, contains the result of three such sets, and from it we find

$$1 \text{ ohm} = \cdot 98598 \frac{\text{earth quadrant}}{\text{second}}.$$

The mean distance between the mean plane of the coils was 15·019 centims.

In Part II three series of experiments are described for different distances between the mean planes of the coils.

In Series A this distance was 15·019 centims.
,, B ,, 18·252 ,,
,, C ,, 26·692 ,,

Different batteries were used.

The following table gives the values of R in earth quadrants/sec. arranged in order of magnitude, with the battery used in each case, the error of each result from the mean, and the percentage error.

In the column headed battery, D stands for an ordinary cylinder Daniell; T for Thomson's sawdust tray Daniell.

Table.

Series.	Battery.	R.	Error.	Percentage error.
B	5 T.	158·106	− ·216	− ·135
A	4 D.	158·168	− ·154	− ·096
A	5 T.	158·231	− ·091	− ·057
C	6 T.	158·238	− ·084	− ·052
B	5 T.	158·303	− ·019	− ·012
C	5 T.	158·332	·010	·006
A	4 D.	158·407	·085	·052
A	2 D.	158·499	·177	·110
C	6 T.	158·611	·289	·181

$$\text{Mean value of R} \quad 158\cdot 322 \frac{\text{earth quadrant}}{\text{second}}.$$

Mean of errors ·125.

Mean of percentage errors ·078.

The value of R in terms of the ohm was found to be

$$160{\cdot}520;$$

the temperature being 12°, the values in the above table have been reduced to this temperature.

From this we find as the value of the ohm—

$$\begin{array}{llll} \text{Series A} & {\cdot}98633\ \dfrac{\text{earth quadrant}}{\text{second}}, & 4 & \text{sets.} \\ \quad ,, \quad \text{B} & {\cdot}98558 \qquad ,, & ,\ 2 & ,, \\ \quad ,, \quad \text{C} & {\cdot}98676 \qquad ,, & ,\ 3 & ,, \end{array}$$

while the mean of the whole set is

$$1\ \text{ohm} = {\cdot}986307\ \frac{\text{earth quadrant}}{\text{second}},$$

this being determined from nine sets of observations. If we include Part I, giving to each observation only half the weight of one of those in Part II (reasons for this are given at full in the paper), we have finally

$$1\ \text{ohm} = {\cdot}986271\ \frac{\text{earth quadrant}}{\text{second}}.$$

The value obtained by Lord Rayleigh in his latest experiments with the rotating coil is

$${\cdot}98651\ \frac{\text{earth quadrant}}{\text{second}}.$$

The experiments have been made at the Cavendish Laboratory, and our thanks are due to Lord Rayleigh for much kind help and many valuable suggestions.

IV. "On a Deep Sea Electrical Thermometer." By C. WILLIAM SIEMENS, D.C.L., F.R.S. Received June 7, 1882.

In the Bakerian Lecture for 1871, which I had the honour of delivering before the Royal Society,* I showed that the principle of the variation of the electrical resistance of a conductor with its temperature might be applied to the construction of a thermometer, which would be of use in cases where a mercurial thermometer is not available.

The instrument I described has since been largely used as a pyrometer for determining the temperatures of hot blasts and smelting furnaces, and Professor A. Weinhold,* using the instrument with a differential voltameter described in my paper referred to, found its indications to agree very closely with those of an air thermometer within the limits of his experiments from 100° to 1,000° Centigrade. I am not aware, however, that any results have been published of its application to measuring temperatures where a much greater degree of accuracy is required, as in the case of deep sea observations. My friend, Professor Agassiz, of Cambridge, U.S., ordered last year for the American Government an instrument designed by me for this purpose, and during the autumn it was subjected to a series of tests on board the United States Coast and Geodetic Survey steamer "Blake," by Commander Bartlett.

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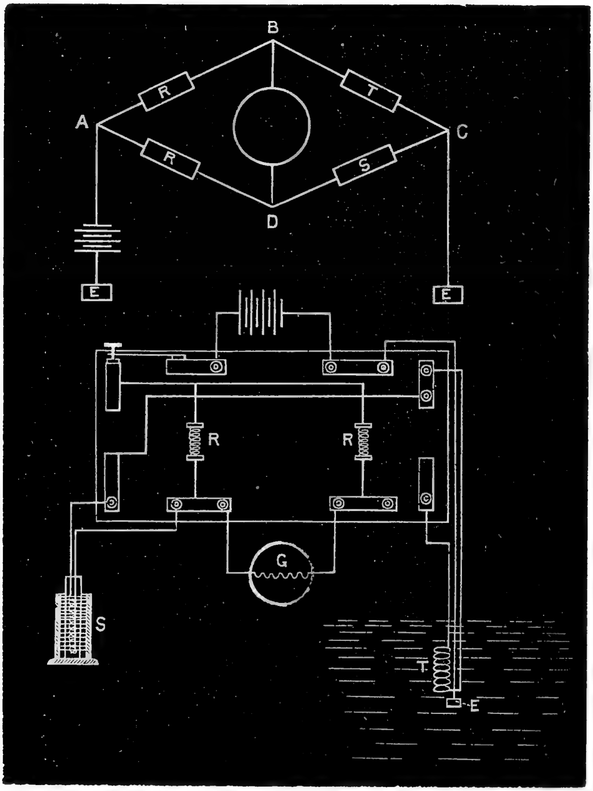
* "Proc. Roy. Soc.," vol. 19, p. 443.



The apparatus consists essentially of a coil of wire T, which is lowered by means of a cable to the required depth; and is coupled by connecting wires to form one arm of a Wheatstone's bridge. The connexions of the bridge are shown in figs. 1 and 2. The arm CD is the comparison coil S made of the same wire as the resistance coil T, and equal to it in resistance. This coil is immersed in a copper vessel of double sides, filled with water, and the temperature of the water is adjusted by adding iced or hot water until the bridge is balanced. The temperature of the water in the vessel is then read by a mercurial thermometer; and this will also be the temperature of the resistance coil.

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* "Annalen der Physik und Chemie," 1873, p. 225.

To avoid the error, which would be otherwise introduced by the leads to the resistance coil, the cable was constructed of a double core of insulated copper wire, protected by twisted galvanised steel wire. One of the copper cores was connected to the arm BC of the bridge, and the other to the arm DC, and the steel wire served as the return earth connexion for both.

The resistance coil and comparison coil were made of silk-covered iron wire ·15 millim. diameter, and each about 432 ohms resistance at a temperature of 66° F. To allow the resistance coil to be readily affected by changes in the temperature of the water, it was coiled on a brass tube with both ends open, allowing a free passage to the water. Sir W. Thomson's marine galvanometer with a mirror and scale was employed to determine the balance of the bridge.

Mr. J. E. Hilgard, assistant in charge of the United States Coast and Geodetic Survey, has sent me the following results of Commander Bartlett's experiments.

The apparatus was set up on board the "Blake," at Providence, in April, 1881, but owing to there being no ice machine on board, only preliminary experiments were made until the following August.

The "Blake" sailed from Charleston on August 4th, running a line over known depths in the current of the Gulf Stream. A 60 lb. sinker used in sounding was attached to the end of the cable near the resistance coil, which was allowed to hang freely below. When well in the strength of the stream a series of temperatures were taken by the Miller-Casella thermometers on the sounding wire, and immediately after the insulated cable was lowered to the surface, and water from the surface placed around the comparison coil on deck. The temperature of the attached thermometer read the same as that determined for the surface by the thermometer attached to the hydrometer case.

Under these conditions the pencil of light from the mirror was on the zero of the scale. During the experiments the vessel was rolling from 10° to 15°, and there was a moderate breeze from south-east. The resistance coil was lowered to five fathoms below the surface, and was allowed to remain five minutes, the circuit being closed, the pencil of light remained at zero. Lowerings were then made to 10, 20, and 30 fathoms, and in each case five minutes were allowed for the resistance coil to assume the temperature of the water, and after adjusting the temperature of the water around the comparison coil, it was allowed to stand five minutes before the final reading was taken.

The rolling of the vessel affected the mirror so as to throw the light about 5° on each side of the zero point when the circuit was open, and nearly the same when closed; but as the deflection was the same on either side it was easy to determine the middle point. While at work in the stream it was necessary to work the engine in order to keep the wire vertical. The jar of the engine, however, affected the mirror to such a degree that readings could only be taken when the engine was stopped.

The Tables I, II, III, IV give the results of the several lowerings.

I.			II.		
Depth in fathoms.	Reading of attached thermometer coil.	Reading of Miller-Casella thermometer.	Depth in fathoms.	Reading of attached thermometer coil.	Reading of Miller-Casella thermometer.
Surface	81°·5	81°·5	Surface	81°·5	81°·5
5	81·5	81·5	30	68·5	
10	76·5	76·5	50	65·25	65
20	70·25	69·5	75	60	
30	69·5	69			
30	68·75	68·75			
III.			IV.		
Surface	83·5	83·5	Surface	84·5	84·5
30	68		30	81	80
50	65·25		50	75·5	
75	60·75		75	61·75	
100	56	54			
150	51				
200	47	47	200	49·5	49·75

On August 10th the "Blake" left Hampton Roads, steaming to the eastward until reaching the meridian of 74° 30′ W., when a sounding was taken, giving a depth of 1,024 fathoms. A serial was taken to a depth of 400 fathoms with two Miller-Casella thermometers, which had been carefully compared with the standard and found to agree at different temperatures. Immediately after the serial with the thermometers the insulated cable was lowered into the sea, and the temperature, by the galvanometer and comparison coil, recorded for the same depths as taken in the first serial. Five minutes was allowed at 5 and 10 fathoms, but there was no deflection of the pencil of light. The temperature of the surface was 76°·5. Having lowered to 15 fathoms, at end of one minute the pencil of light was 9° to the left of zero on the scale. At the end of five minutes it was 22°, and at the end of ten minutes still 22°. A number of experiments were made with regard to the time necessary for the resistance coil to assume the temperature of the water. Five minutes was decided on as being necessary and sufficient, and was adopted in all succeeding lowerings.

The first lowering was to 400 fathoms, the temperature at that depth being 40°. The cable was then reeled in to 200 fathoms, when the current was made. There was found to be no deflection, the temperature of the water in the copper vessel having risen from 40° to 43°·5. This temperature agreed with that at 200 fathoms when lowering to the same depth.

During the experiments there was a light south-east breeze, and a very smooth sea. They lasted from 7.18 P.M. until 1.30 A.M., but special care was taken with every reading, and it is probable that fifteen minutes would be a fair average time for each observation with the electrical apparatus.

The results are given in the Table.

I.			II.		
Depth in fathoms.	Reading of attached thermometer coil.	Reading of Miller-Casella thermometer.	Depth in fathoms.	Reading of attached thermometer coil.	Reading of Miller-Casella thermometer.
Surface	76°·5	76°·5	30	54°	54°
5	76·5	76·5	50	54·25	53·5
10	76·5	76	100	50·5	50·5
15	69	68	150	46·5	46·5
20	58	58	200	43·5	43·5
30	54·25	54			
50	54·25	53·5			
75	52·5	52·5			
100	51	50·5			
150	46	46·5			
200	43·5	43·5			
300	40·5	40·5			
400	40	40			

Early on the morning of August 12th another serial to 800 fathoms was taken with the Miller-Casella thermometers, and immediately after with the electrical apparatus. Several readings were taken from the surface to 100 fathoms, and then the coil was reeled out to 800 fathoms, and the readings taken as it was drawn up.

Depth in fathoms.	Reading of attached thermometer coil.	Reading of Miller-Casella thermometer.	Depth in fathoms.	Reading of attached thermometer coil.	Reading of Miller-Casella thermometer.
Surface	76°	76°	Surface	77°·5	77°·5
5	76	75·25	5	76·25	75·25
10	73·5	69	10	75·5	69
15	61·25	68	15	66·5	63·5
20	55·5	59	20	58	57
30	51	52·5	30	51·5	51·5
50	53·75	52	50	54·5	53·5
75	52·5	52·5	75	53·5	52·5
100	50	49·5	100	51	49·5
			125	48·5	
			150	46·5	46
			200	43·5	43·25
			300	40·5	40·75
			400	40	39·75
			500	39·25	39
			600	38·75	38·75
			700	38·5	38·5
			800	38·5	38·5

In the last series of observations in reeling back the cable, the temperature at 50 fathoms was 54°·5, and fell to 51°·5 at 30 fathoms. Immediately after another series was taken with the Miller-Casella thermometer, and the same increase of temperature from 30 to 50 fathoms was observed. The cable was lowered three separate times to 50 fathoms, and the readings being taken both when lowering and reeling in with the following results:—

Depth in fathoms.	Reading of attached thermometer coil.	Reading of Miller-Casella thermometer.	Depth in fathoms.	Reading of attached thermometer coil.	Reading of Miller-Casella thermometer.
Surface	77°·5	77°·5		°	°
20	57·25	57	30	51·75	52
30	52·25	52	50	54·5	53·5
50	55·25	53·5	75	53	52·5
20	57·75	57			
30	52·75	52			
50	54·75	54			
75	53	52·5			

During the above experiments the sea was perfectly smooth, with no wind. The ship's engines were not used at all, the vessel lying almost motionless in the water. The temperature of the comparison coil was reduced by water from a carafe, the water contained therein being frozen by a Carré ice machine. Two carafes were prepared at a time, and there was plenty of time to keep one constantly at hand.

In order to allow the Miller-Casella thermometers to record the high temperature of 50 fathoms in the last series, they were lowered very rapidly to that depth, and after eight minutes reeled back at the rate of 200 fathoms per minute, so that the minimum side had not time to assume a lower temperature.

The cable was led from a large reel through an 18-inch leading block, and was lowered and reeled in very slowly, and without jerks.

It may be noted in the above Tables that the two instruments gave precisely the same readings at positions of maximum or minimum temperature, but that in intermediate positions the electrical thermometer, in almost every instance, gave a higher reading. This discrepancy may be accounted for, I think, by the circumstance that the electrical thermometer gives the temperature of the water actually surrounding the coil at the moment of observation, whereas the reading of the Miller-Casella instrument must be affected by the maximum or minimum temperatures encountered in its ascent or descent, which may not coincide with that at the points of stoppage. A strong argument in favour of the electrical instrument for geodetic and meteorological purposes has thus been furnished.

V. "On the Coxal Glands of Scorpio hitherto undescribed and corresponding to the Brick-red Glands of Limulus." By E. RAY LANKESTER, M.A., F.R.S., Jodrell Professor of Zoology in University College, London. Received May 25, 1882.

In my essay entitled "Limulus an Arachnid,"[*] I have mentioned Dr. Packard's discovery of the "brick-red glands" of Limulus, situated at the junction of the coxæ of the prosomatic limbs with the body in the following terms:—"It is true that Packard has assimilated a brick-red coloured structure occurring at the base of the cephalothoracic limbs of Limulus to a shell-gland or renal organ. In this I cannot agree with him. It is not even apparent, at present, that this brick-red organ, which I have examined, is of a glandular nature at all."

Dr. Packard first described these glands in 1874, and figured them subsequently in his valuable memoir on the "Anatomy, Histology, and Embryology of Limulus Polyphemus," published in the Anniversary Memoirs of the Boston Society of Natural History, 1880.

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[*] "Quart. Journ. Micr. Sci.," 1881.

Dr. Packard observes—"These glands are quite large and apparently of some physiological importance, and are easily found, as they are conspicuous from their bright red colour, causing them to contrast decidedly with the dark masses of the liver, and the yellowish ovaries or greenish testes, near which they are situated. The glands are bilaterally symmetrical, one situated on each side of the proventricle and stomach, and each is entirely separated from its fellow. Each gland consists of a stolon-like mass extending along close to the great collective vein, and attached to it by irregular bands of connective tissue, which also hold the gland in place. From this horizontal mass four vertical branches arise, and lie between and next to the partitions at the base of the legs, which divide the latero-sternal region of the cephalothorax into compartments. The posterior of these four vertical lobes accompanies the middle hepatic vein from its origin from the great collective vein, and is sent off opposite the insertion of the fifth pair of feet. Halfway between the origin of the vein and the articulation of the limb to the body, it turns at a right angle, the ends of the two other lobes passing a little beyond it, and ends in a blind sac, less vertical than the others, slightly ascending at the end, which lies just above the insertion of the second pair of feet. The two middle lobes are directed to the collective vein. Each lobe is somewhat flattened out, and lies close to the posterior wall of the compartment in which it is situated, as if wedged in between the wall and the muscles between it and the anterior portion of the compartment. Each lobe also accompanies the bases of the first four tegumentary nerves."

I can fully confirm the accuracy of this careful description of the naked-eye appearance and situation of these glands. I am also in agreement with Dr. Packard, when he states that these glands have no opening into the great veins, and, like him, I have as yet been unable to detect the situation of their opening to the exterior.

Dr. Packard's description of the minute structure of these brick-red bodies is such as to have led me to doubt the correctness of his conclusion that they are glands and more especially renal glands. At the time when I wrote to that effect I had only made dissections showing their position and relations in two specimens of Limulus. I have since been able to obtain perfectly fresh specimens of the brick-red glands from a Limulus killed for the purpose, and having hardened them in absolute alcohol, I have prepared and examined sections demonstrating their minute structure.

This does not agree with the description given by Dr. Packard, whose account of the minute structure of these bodies led me to doubt their glandular nature.

Dr. Packard states that "the four lobes end in blind sacs and have no lumen or central cavity," and in the next paragraph somewhat inconsistently remarks "each lobe when cut across is oval, with a yellowish interior and a small central cavity." He states that the gland is "dense though yielding, and on this account hard to be cut with the microtome," and appears to have confined his observations accordingly to preparations of the fresh gland teased. Various kinds of cells, from a "cortical" and a "medullary" substance of the gland, are described and figured, but it does not seem to be possible to bring these results of "teasing" into relation with what I have observed in sections taken in various directions across the lobes of the gland, and stained and mounted in the usual way in balsam.

I find that each of the quadrilobate glands, which I should propose to call the right and the left "coxal glands," is essentially a sac, lined with a characteristic glandular epithelium, the lumen of the sac being cut up into a number of inter-communicating passages by the production of the inner surface of the sac into very numerous and far-reaching trabeculæ. The gland-cells which clothe these trabeculæ are remarkable for their round, well-defined nuclei, and for the possession of a peculiar differentiation of the substance of the cells near their free surface, which has at first sight the appearance of a very thick cuticle.

A more detailed account of the structure of these cells may be deferred for the present. My object now is to point out that in the Scorpions there exists a similar pair of large coxal glands, having essentially the same structure and position as the coxal glands of Limulus. I was led to look for the existence of such glands by the hypothesis that Scorpio and Limulus are very closely related members of the class Arachnida; and it will, I think, be conceded that the discovery of the existence of such corresponding organs goes a long way towards confirming the conclusion as to the close affinity of the two animals, to which I had been led by the observation in them of numerous other structural coincidences.

The coxal glands of the Scorpions are very large and prominent structures, each attaining the size of a dried pea in a large Indian scorpion of five inches in length. They are placed as in Limulus at the junction of the coxæ of the ambulatory limbs with the body (fig. 1. B.). They do not send lobes forward corresponding to the second, third, and fourth of the six limbs of the prosoma, but are oblong white bodies resting upon the sternal prolongations of the hinder limbs (fifth and sixth) on each side. Posteriorly each gland rests against the ingrowing chitinous wall (fig. 1. C.) of the coxa of the last limb of the prosoma which forms the posterior cornu of the entosternite, but the mass of the gland lies in the hollow of the sternal prolongation of the fifth limb, and is attached to it by a triangular outgrowth which I shall provisionally speak of as a duct, though I have not at present succeeded in finding any external aperture corresponding to these coxal glands of the Scorpion, any more than I have in the case of the similar glands of Limulus.

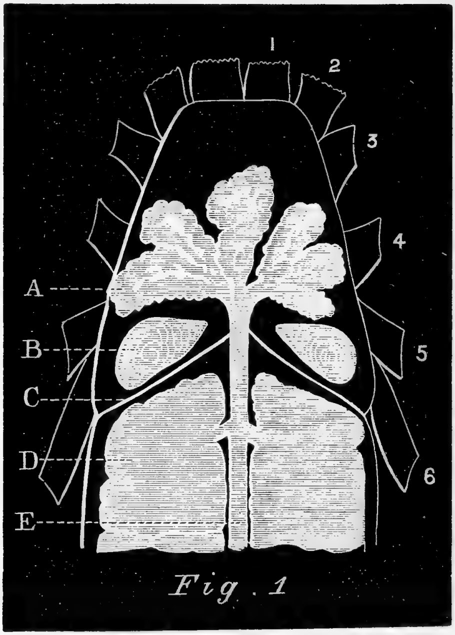

Fig. 1

Diagram of the anterior portion of a Scorpion's body, to show the position of the coxal glands.

A. Anterior glandular cæca of the alimentary canal (salivary glands of Newport and Blanchard, *not* of Dufour). These are drawn of smaller size than natural, and are turned forward so as to expose the coxal glands.
B. The coxal gland of the left side.
C. Fibrous septum (diaphragm of Newport) formed by the posterior cornua of the entosternite.
D. Glandular cæca of the alimentary canal (so-called "liver").
E. Axial portion of the alimentary canal.
1 to 6. The six pairs of limbs of the prosoma.

These oblong, almost egg-shaped, glistening white bodies have not altogether escaped the notice of previous students of the anatomy of Scorpions; but owing to the fact that spirit-specimens have been used by most naturalists who have dissected Scorpions, and that spirit fails to preserve the softer tissues of a whole Scorpion in a fit state for observation, the nature of these glands has been misunderstood.

I have been enabled to dissect freshly-killed specimens of the large Indian scorpion, *Sc. cyaneus* (allied to the species called *Buthus**

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* Not the genus *Buthus* of Leach, but of Gervais, identical with *Heterometrus* of

Kochii and *Buthus afer* by some authors), through the kind exertions of my friend Dr. Henry Trimen, Director of the Royal Gardens at Peridenya, Ceylon, also of the fine North African *Androctonus funestus* (Ehrenberg spec.), of which I received specimens in a living state from Algeria through the courteous intervention of Professor Carl Vogl, of Geneva; and, lastly, of *Scorpio Italicus*, Roessel, and *Sc. Carpathicus*, Linn., the common little Italian scorpions (not to be confused with the larger yellow Spanish *Sc. Europæus* of Linnæus, which is an Androctonus closely allied to *A. funestus* and often called *A. occitanus*), for which I have to thank Mr. Gibson Carmichael. When the prosomatic carapace is removed from one of these Scorpions recently killed, the white oviform coxal glands are seen in the position described, right and left of the alimentary tract. The anterior glandular cæca (fig. 1. A.) of the alimentary tract, called salivary glands by Newport, rest upon the coxal glands and hide them to a certain extent. This proximity has led to the notion that the coxal glands are connected with the alimentary canal.

Newport in the plates illustrating his masterly description of the circulatory and nervous system of the Scorpion, published in the "Phil. Trans." nearly forty years ago (1843), has figured these bodies, but has not described them in the text of his work. In the description of the plate they are spoken of as "lateral appendages of the thoracic portion of the canal, (?) gizzard (?)." The accuracy and completeness of Newport's account of the vascular and nervous systems is worthy of profound admiration, when it is remembered that he had only specimens preserved in alcohol to deal with. At the same time this condition of his specimens accounts for the incorrectness of his conclusions as to the very soft and decomposable glandular structures.

Leon Dufour ("Mémoires de l'Institut," Tom. 14, 1856) has also described and figured the coxal glands in *Androctonus occitanus* (*Scorpio Europæus*) the large yellow scorpion of southern France and Spain. Dufour had the advantage of using freshly-killed specimens, but his account of the anatomy of this species appears to me to be, nevertheless, curiously inaccurate in many important particulars. He very properly does not consider the glandular cæca of the most anterior portion of the alimentary canal as "salivary glands" as did Newport, but recognises the identity of their structure with that of the large glandular masses filling up the mesosoma which have been termed "hepatic," and accordingly describes the "salivary glands" of Newport as the anterior or cephalothoracic lobes of the liver.

He, however, describes the pair of coxal glands as "salivary

Ehrenberg. The confusion of nomenclature among the Scorpions is very great. Peters ("Berlin Monatsbericht," 1861, p. 510) has given the best systematic arrangement of the sub-genera.

glands," and this, notwithstanding that he has accurately recognised the absence of any duct connecting them with the alimentary canal. He *figures* such a duct, but remarks in the text of his memoir, that these apparent ducts are really ligaments.

He further figures and describes the broad triangular offset from the anterior and inferior margin of the gland which appears to me to be in all probability its true duct,* and gives to this the name of "fleshy pedicle."

Dufour observed and figured something of the minute structure of the coxal (his "salivary") gland. He shows that its smooth white surface is marked by winding lines of a labyrinthine arrangement, but erroneously attributes their existence to a coiled tubular structure.

My own observations on the minute structure of the coxal glands of the Scorpions dissected by me are briefly as follows: the three species agreeing in essentials. Each gland is a sac; the labyrinthine markings seen on the surface being due to the existence of labyrinthine trabeculæ which rise up from the inner surface of the sac, and break up its lumen into numerous narrow passages as is the case in the coxal glands of Limulus. An injection of freshly precipitated lead chromate forced into the gland did not escape from it by any duct, but distended the triangular "pedicle," which I consider as probably the duct, though I have as yet failed to find any pore corresponding to it on the outer surface of the coxa or of the sternum.

Coxal glands taken from freshly killed Scorpions and placed in absolute alcohol, and subsequently stained with picrocarmine or hæmatoxylin, and cut into sections in the usual manner, showed that the gland is a sac with its wall folded inwards, so as to form numerous trabeculæ, clothed with a remarkable epithelium. The cells of this epithelium are much larger than those of the coxal glands of Limulus, but agree with them in presenting a differentiation of the cortical substance of the cell. Each cell of the epithelium of the scorpion's coxal gland presents in optical section a complete cortical ring of bright dense-looking substance surrounding a transparent protoplasm, in the centre of which is the nucleus. The dense cortical substance of each cell appears to be finely striated; the striæ radiating from the medullary substance towards the surface of the cell. This striated structure recalls to mind the striated structure of the cells of the leech's nephridium, and of the mammalian kidney.

The trabeculæ on which these cells rest are cavernous, being filled with blood. The blood spaces so formed are larger than the spaces left between the projecting trabeculæ. The latter are the true "lumen" of the gland. The structure of the central portion of the coxal gland of the Scorpions differs from that of its periphery, to which the previous description refers.

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* *June 24th*, 1882.—I am indebted to the skill of my assistant, Mr. A. G. Bourne, B.Sc., for some complete series of sections through small Scorpions, the study of which has led me to doubt whether these glands have any duct. Their exact nature and function require further investigation, with which I am engaged.

Conclusion.—It does not seem possible to doubt that the coxal glands of the Scorpions and of Limulus are homologous structures. Though no external opening has been found as yet, in either the one case or the other, it is possible that such an opening exists. Though glands in a similar position (at the bases of the limbs or jaws) are found in other Arthropoda, there are none known which agree so closely in position and structure with either the coxal glands of Limulus, or of Scorpio, as these do with one another. Possibly such coxal glands are in all cases the modified and isolated representatives of the complete series of tubular glands (nephrida) found at the base of each leg in the archaic Arthropod, *Peripatus.*

P.S.—I may add that since writing the above, I have found a similar pair of glandular organs in a large South American Mygale, which I received from the Zoological Society on the day of its death, in the Insect House at Regent's Park. The coxal glands in Mygale are elongated and lobed as in Limulus. They rest on the dorso-lateral region of the entosternite.

VI. "Note on the Differences in the Position of the Ganglia of the Ventral Nerve-cord in three Species of Scorpion." By E. Ray Lankester, M.A., F.R.S., Jodrell Professor of Zoology in University College, London. Received May 25, 1882.

No one who is acquainted with the researches of George Newport can doubt the general accuracy of his description of the nervous system of an "Androctonus." Very probably the Scorpion which he made use of for his researches on the nervous system was *Androctonus funestus*, Ehr., the same which I have received in the living condition from North Africa.

The accuracy of Newport's description in a very material point, and one which refers to a very obvious feature, has been called in question by Leon Dufour. Dufour dissected a species closely allied to *A. funestus*, one which is little more than a small European variety of it, namely, *A. occitanus.* Yet he gives an account of the disposition of the ganglia of the ventral nerve-cord, and of the innervation of the four pairs of pulmonary sacs, which is widely different from that of Newport.

I am able to confirm the general accuracy of Newport's account by my dissections of *A. funestus*, and have been in much doubt as to whether Dufour's account ought to be considered as erroneous, or as due to a difference in the species which he dissected. Whilst thus doubtful I discovered that the *Scorpio* (*Buthus* of some authors) *cyaneus* of Ceylon presents a disposition of the ganglia of the ventral nerve-cord and an innervation of the pulmonary sacs, which differs both from that of *A. funestus* as described by Newport, and of *A. occitanus*, as described by Dufour. Further I have found that the *Scorpio Italicus* (as also the allied *Sc. Carpathicus*) has a disposition of these parts agreeing with that observed by me in the *Scorpio cyaneus* of Ceylon. Thus I am led to think it possible—though I cannot say that I think it probable—that the difference in the accounts given by Newport and Dufour of the two species of Androctonus is due to specific variation.

The disposition observed by Newport in his Androctonus, which I can confirm so far as *A. funestus* is concerned,* is as follows:—

Tracing the nerve-cord from the ganglionic mass in the cephalothorax (or prosoma, as I prefer to call it), we find in the mesosoma (the broad so-called abdomen of authors) a ganglion in the segment containing the *second* pair of lungs, a second ganglion in the segment containing the fourth pair of lungs, and a third ganglion in the lungless segment which succeeds the segment containing the fourth pair of lungs. These three ganglia do not all supply the structures adjacent to them: each sends off two lateral nerves and a median inferior nerve. The latter nerve was not seen by Newport, but is rightly described by Dufour. Dufour also observed what Newport failed to do, viz., that the nerve-cord itself is double. The *first and the second* pair of lungs, as well as the adjacent regions of the segments in which they lie, are supplied by two pairs of nerves, which descend from the ganglionic mass in the prosoma (cephalothorax). The ganglion adjacent to the second pair of lungs supplies by its lateral nerves, *not* that pair of lungs, *but the third pair* of lungs. The next ganglion supplies the adjacent fourth pair of lungs, whilst the ganglion in the segment following that containing the fourth pair of lungs supplies the muscles adjacent to it, there being no lungs in this segment.

In the Ceylon scorpion, *S.* (*Buthus*) *cyaneus*, and in *Sc. Italicus* (Raes), I find, on the other hand, the following arrangement. There are three ganglia in the broad abdomen, one placed very close to the prosoma in the segment belonging to the first pair of lungs, a second placed in the segment corresponding to the third pair of lungs, and a third in the lungless segment. Further, *the distribution of the lateral nerves given off by the ganglia* is quite different from what we find in *Androctonus funestus*.

In *S. cyaneus* of Ceylon only *one* (the first) pair of lung-sacs (and

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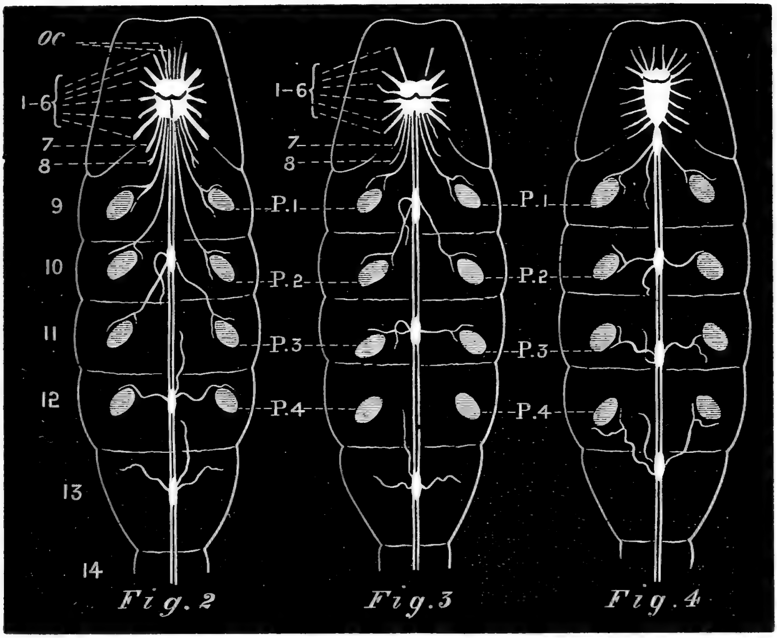
* *June 24th.*—Newport's drawing, however, represents the middle of the three ganglia as lying in the third lung-segment, instead of the fourth.



Figures 2, 3, 4. Diagrams showing the disposition of the ganglia and chief nerves in Scorpions.

Figure 2 represents the arrangement found in *Androctonus funestus.* It is, except for the position of the middle ganglion, the same as that described by Newport, the median nerve given off from each ganglion of the cord, and the *two* strands (instead of *one*) which constitute the cord being additions to that author's results.

Figure 3 represents the arrangement found in the large tropical *Scorpio* (Buthus, or Heterometrus of Ehrenberg) *cyaneus* of Ceylon, and *Sc. Kochii* of India, and also in the small European *Scorpio Italicus*, Roes, and *Scorpio Carpathicus*, Linn.

Figure 4 is constructed from the figure given by Leon Dufour, of *Androctonus occitanus*—the yellow Scorpion of Spain and Southern France. In those points in which this figure differs from fig. 2, it seems probable that Dufour is incorrect, but *possibly* he is not altogether so.

oc., nerves to central and lateral eyes.
1—6, nerves to the six limbs of the prosoma.
7, nerve to the genital operculum (first segment of mesosoma).
8, nerve to the pecten (second segment of mesosoma).
9—12, four hinder segments of the mesosoma.
13, 14, two first segments of the metasoma.
P^1 to P^4, the four pairs of pulmonary sacs.

the adjacent parts of the segment in which it lies) is supplied from the great ganglion of the prosoma. The ganglion lying in this, the first lung-bearing segment, supplies the second lung-bearing segment.

The third lung-bearing segment is supplied by the ganglion which lies in that segment. The fourth lung-bearing segment has no ganglion lying in it, and appears to depend entirely for its nerve-supply on the median nerve given off by the ganglion which, as in Androctonus, is placed in the next following segment.

These relations can only be understood by the aid of drawings. The woodcut shows diagrammatically in three figures, fig. 2 the disposition in *Androctonus funestus* (agreeing essentially with Newport's description), fig. 3 the disposition in *Scorpio cyaneus* of Ceylon, fig. 4 the disposition described by Dufour. This last diagram has been constructed from Dufour's drawing. It is without doubt erroneous in important particulars relative to the large ganglion of the prosoma, and is very probably erroneous in regard to other particulars.

It is worthy of attention as tending to associate the European scorpions of the sub-genus Euscorpius with those of the type of *Buthus afer*, to which *Sc. cyaneus* of Ceylon belongs, that in *Scorpio Italicus* and *Scorpio Carpathicus*, I have found the same disposition of the ganglia and of the primary branches of the ventral nerve-cord as that drawn in fig. 3. The only difference observed was that the ganglia in these European species are all a *little* further forward, so as to lie close to the anterior limit of the segments in which they occur.

We thus find that an important anatomical difference obtains between the Scorpions with triangular sternum (Androctoni) and the Scorpions with pentagonal sternum (Euscorpii, Buthi, &c.). Whether the Scorpions with band-like sternum (Telegoni) differ from or agree with either of these types in respect of their nervous system, has yet to be discovered.

VII. "On the Specific Heat and Heat of Transformation of the Iodide of Silver, AgI, and of the Alloys $Cu_2I_2.AgI$, $Cu_2I_2.2AgI$, $Cu_2I_2.3AgI$, $Cu_2I_2.4AgI$, $Cu_2I_2.12AgI$, $PbI_2.AgI$." By Professor M. BELLATI and Dr. R. ROMANESE, Professors in the University of Padua. Communicated by Professor A. W. WILLIAMSON, For. Sec. R.S. Received June 7, 1882.

(Abstract.)

The authors' calorimetric investigation refers to substances which have been already studied by Mr. G. F. Rodwell ("Proc. Roy. Soc." vol. 32) as to their expansion and contraction by heat.

After having detailed the method of experimenting and tabulated the results of determinations for each substance, the authors recapitulate the results in the following table:—

Composition of the substance.	Percentage of AgI.	θ_1.	θ_2.	c.	c_1.	λ.
		C.	C.			
AgI	100·0	142°	156°·5	0·054389 + 0·0000372(T + t)	0·0577	6·25
$Cu_2I_2.12AgI$	83·1	95	228	0·05882 (from 16° to 89°)....	0·0580	8·31
$Cu_2I_2.4AgI$	71·2	180	282	0·056526 + 0·0000410(T + t)	0·0702	7·95
$Cu_2I_2.3AgI$	65·0	194	280	0·059624 + 0·0000280(T + t)	0·0726	7·74
$Cu_2I_2.2AgI$	55·3	221	298	0·061035 + 0·0000295(T + t)	..	7·88
$Cu_2I_2.AgI$..	38·2	256	324	0·063099 + 0·0000260(T + t)	..	8·67
$PbI_2.AgI$ ..	33·8	118	144	0·047458 + 0·0000026(T + t)	0·0567	2·556

In this table θ_1 and θ_2 are the temperatures at which the structure change commences and finishes, according to Mr. Rodwell's results, c denotes the mean specific heat of the substance between t and T for temperatures below θ_1; c_1 is the specific heat for temperatures beyond θ_2; and λ is the heat absorbed by the unit weight of the substance in consequence of modification of structure. The relative accuracy of certain results and some probable conclusions are finally discussed.

VIII. "(I.) On a Tangential Property of Regular Hypocycloids and Epicycloids. (II.) On Theorems relating to the Regular Polyhedra which are analogous to those of Dr. Matthew Stewart on the Regular Polygons." By HENRY M. JEFFERY, F.R.S. Received June 3, 1882.

(Abstract.)

I. *On a Tangential Property of Regular Hypocycloids and Epicycloids.*

1. The following theorems will be established :—

(A.) The product of the perpendiculars drawn from all the cusps in each of these roulettes on any tangent is a function of the perpendicular only, which is drawn on the same tangent from the centre of the fixed circle, on which the roulette is generated. Consequently

(B.) The sums of the cotangents of the angles which any tangent to the roulette makes with the vectors drawn from the cusps to the point of contact is a function of the cotangent of the angle made by the vector drawn from the centre with the same tangent line, and of the perpendicular drawn from the centre.

2. These propositions are extended to spherical geometry, and their dual forms stated both for planimetry and spherics.

3. A proof will be hence derived for two general theorems on regular polygons given by Dr. Matthew Stewart (Props. 39, 42), of which many others are special cases.

4. These propositions will first be elucidated by examples.

In the case of the regular hypocycloids, we may consider them as referred to line-coordinates, the polygon of reference being formed by joining the (n) cusps.

$$\text{If} \quad n=3, \quad p_1p_2p_3=p^3=\frac{1}{3^3}(p_1+p_2+p_3)^3 \text{ or } p_1^{\frac{1}{3}}+p_2^{\frac{1}{3}}+p_3^{\frac{1}{3}}=0.$$

$$n=4, \quad p_1p_2p_3p_4=p^4=\frac{1}{4^4}(p_1+p_2+p_3+p_4)^4.$$

$$n=5, \; p_1p_2p_3p_4p_5=p^5-\left(5-\frac{5^3}{3^3}\right)\left(\frac{a}{2}\right)^2p^3.$$

Consequently by differentiating with respect to θ (the inclination of a perpendicular on a tangent to the initial line) in the several cases on both sides (Besant, "On Roulettes," § 7)—

$$\text{If} \quad n=3, \; \cot\alpha_1+\cot\alpha_2+\cot\alpha_3=3\cot\alpha,$$

$$n=4, \quad \Sigma\cot\alpha_m=4\cot\alpha,$$

$$n=5, \quad \Sigma\cot\alpha_m=\frac{5p^2-d}{p^2-d}\cot\alpha,$$

where d is a constant, and $\alpha_1, \alpha_2, \ldots \alpha$ denote the angles made by the tangent with the vectors from the cusps and centre.

5. In the epicycloid the same polygon of reference is used.

$$\text{If} \quad n=1, \; \tfrac{1}{4}a^2p_1=\frac{p^3}{3^3}, \text{ the unicuspid cardioid,}$$

$$n=2, \quad \tfrac{1}{4}a^2p_1p_2=\frac{p^4}{2^4},$$

$$n=3, \; \tfrac{1}{4}a^2p_1p_2p_3=\frac{3^5}{5^5}p^5-\tfrac{1}{4}a^2p^3\left(\frac{3^3}{5^2}-1\right).$$

$$\text{Consequently, if} \quad n=1, \quad \cot\alpha_1=3\cot\alpha,$$

$$n=2, \; \cot\alpha_1+\cot\alpha_2=4\cot\alpha.$$

The general theorems will now be established.

6. Let these perpendiculars from the cusps be expressed by tangential polar coordinates, when the initial line is drawn from the centre to a cusp:—

$$p_1p_2\,..\,p_n=(p-a\cos\theta)\left\{p-a\cos\left(\frac{2\pi}{n}+\theta\right)\right\}\,..\,\left\{p-a\cos\left(\frac{2n-2}{n}\pi+\theta\right)\right\}.$$

The artifice used by Gregory in a kindred-question ("Math.

Journ.," vol. iii, p. 145) is here adopted. Let $x^2+y^2=p$, $2xy=a$. Cotes' theorem is thereby applicable to determine the product.

$$x^{2n}+y^{2n}-2x^ny^n\cos n\theta=(x^2-2xy\cos\theta+y^2)\left\{x^2-2xy\cos\left(\frac{2\pi}{n}+\theta\right)\right\}\dots$$

We must next express $x^{2n}+y^{2n}$ in terms of p, a. The expression, which is closely allied to the expansion of $\cos n\theta$, is given in Todhunter's "Theory of Equations," p. 183.

Since $$(1-zx^2)(1-zy^2)=1-z(x^2+y^2-zx^2y^2),$$

if we take the logarithm of both sides, and select the coefficient of z^n,

$$x^{2n}+y^{2n}=p^n-n\left(\frac{a}{2}\right)^2p^{n-2}+\frac{n(n-3)}{1.2}\left(\frac{a}{2}\right)^4p^{n-4}-\dots$$

It is also necessary to obtain the ascending series—

$$i^{\frac{n}{2}}\tfrac{1}{2}(x^{2n}+y^{2n})=\left(\frac{a}{2}\right)^n-\tfrac{1}{2}n^2\left(\frac{a}{2}\right)^{n-2}\left(\frac{p}{2}\right)^2+\frac{n^2(n^2-2^2)}{1.2.3.4}\left(\frac{a}{2}\right)^{n-4}\left(\frac{p}{2}\right)^4-\dots$$

if n be an even integer,

$$i^{\frac{n-1}{2}}\tfrac{1}{2}(x^{2n}+y^{2n})=n\left(\frac{a}{2}\right)^{n-1}\frac{p}{2}-\frac{n(n^2-1)}{1.2.3}\left(\frac{a}{2}\right)^{n-3}\left(\frac{p}{2}\right)^3+\dots$$

if n be an odd integer.

7. The polar tangential equations to the regular hypocycloids and epicycloids are

$$p=\frac{n\mp2}{n}a\sin\frac{n}{n\mp2}\left(\frac{\pi}{2}\mp\theta\right),$$

where a is the radius of the fixed circle, and bears to the radius of the rolling circle the ratio $n:1$ (Besant, "On Roulettes," § 14).

If we take the upper sign, and write—

$$\sin\chi=\frac{np}{n-2},\quad\text{where } (n-2)\chi=n\left(\frac{\pi}{2}-\theta\right).$$

Hence $-2\cos n\theta$

$$=2\cos(n-2)\left(\frac{\pi}{2}-\chi\right),$$

$$=(2\sin\chi)^{n-2}-(n-2)(2\sin\chi)^{n-4}+\frac{(n-2)(n-5)}{1.2}(2\sin\chi)^{n-6}-\dots$$

$$=\left\{\frac{2np}{(n-2)a}\right\}^{n-2}-(n-2)\left\{\frac{2np}{(n-2)a}\right\}^{n-4}+\dots$$

Hence the general theorem is established for hypocycloids.

$$p_1p_2\ldots p_n=p^n-\left(\frac{a}{2}\right)^2p^{n-2}\left\{n-\left(\frac{n}{n-2}\right)^{n-2}\right\}$$

$$+\left(\frac{a}{2}\right)^4p^{n-4}\left\{\frac{n(n-3)}{1\,.\,2}-(n-2)\left(\frac{n}{n-2}\right)^{n-4}\right\}$$

$$-\left(\frac{a}{2}\right)^6p^{n-6}\left\{\frac{n(n-4)(n-5)}{1\,.\,2\,.\,3}-\frac{(n-2)(n-5)}{1\,.\,2}\left(\frac{n}{n-2}\right)^{n-6}\right\}+\ldots$$

The series ends with the term involving p^3 or p^4, according as n is odd or even, as may be seen by examining the expansion of $\cos n\theta$ in an ascending series.

8. The equation to the regular epicycloid is

$$\sin\chi=\frac{np}{(n+2)a},\quad\text{where }(n+2)\chi=n\left(\frac{\pi}{2}+\theta\right).$$

Hence the formula for this epicycloid is

$$\frac{a^2}{4}p_1p_2\ldots p_n=\left(\frac{n}{n+2}\right)^{n+2}p^{n+2}-\left(\frac{a}{2}\right)^2p^n\left\{(n+2)\left(\frac{n}{n+2}\right)^n-1\right\}$$

$$+\left(\frac{a}{2}\right)^4p^{n-2}\left\{\frac{(n+2)(n-1)}{1\,.\,2}\left(\frac{n}{n+2}\right)^{n-2}-n\right\}$$

$$-\left(\frac{a}{2}\right)^6p^{n-4}\left\{\frac{(n+2)(n-2)(n-3)}{1\,.\,2\,.\,3}\left(\frac{n}{n+2}\right)^{n-4}-\frac{n(n-3)}{1\,.\,2}\right\}+\ldots$$

By reversing the order in expanding $\cos n\theta$, it appears that no terms involving p^2, p, p^0 occur.

12. Proof of Stewart's theorems.

Since the formulæ of § 6 are general, they apply to a parallel line, on which the perpendiculars are drawn.

$$(p_1+x)(p_2+x)\ldots(p_4+x)$$

$$=(p+x)^n-n\left(\frac{a}{2}\right)^2(p+x)^{n-2}+\frac{n(n-3)}{1\,.\,2}\left(\frac{a}{2}\right)^4(p+x)^{n-4}-\ldots\equiv\mathrm{U}.$$

The sums of the several powers of p_1, p_2, . . . are obtained by taking the logarithm, and differentiating on both sides—

$$\frac{n}{x}-\frac{1}{x^2}\Sigma_1+\frac{1}{x^3}\Sigma_2-\frac{1}{x^4}\Sigma_3+\ldots=\frac{1}{\mathrm{U}}\frac{d\mathrm{U}}{dx}.$$

(This last quotient is remarkable, as it shows the matrix out of which these theorems of Dr. Stewart arose.)

$$=\frac{n}{p+x}\left\{1+\frac{2}{1}\left(\frac{a}{2}\right)^2\frac{1}{(p+x)^2}+\frac{4\,.\,3}{1\,.\,2}\left(\frac{a}{2}\right)^4\frac{1}{(p+x)^4}+\ldots\right\}$$

$$=\frac{n}{x}\left\{1-\frac{p}{x}+\frac{p^2}{x^2}-\frac{p^3}{x^3}+\dots\right\}$$

$$+\frac{2}{1}\left(\frac{x}{2}\right)^2\frac{n}{x^3}\left\{1-\frac{3}{1}\frac{p}{x}+\frac{4\,.\,3}{1\,.\,2}\frac{p^2}{x^2}-\dots\right\}$$

$$+\frac{4\,.\,3}{1\,.\,2}\left(\frac{a}{2}\right)^4\frac{n}{x^5}\left\{1-\frac{5}{1}\frac{p}{x}+\frac{6\,.\,5}{1\,.\,2}\frac{p^2}{x^2}-\dots\right\}+\dots$$

By equating the several powers of $\frac{1}{x}$, the several sums of powers are found.

$$\Sigma(p_r)^m=n\left\{p^m+\frac{m(m-1)}{2^2}p^{m-2}a^2+\frac{m(m-1)(m-2)(m-3)}{2^2\,.\,4^2}p^{m-4}a^4+\,.\right\}.$$

This is the dual of Stewart's Prop. 40.

13. If in § 12 the line touch the circle, so that $p=a$, $\Sigma(p_r)^m$ becomes

$$n\frac{(2m-1)(2m-3)\dots 3\,.\,1}{m(m-1)\dots 2\,.\,1}a^m \quad . \quad . \quad \text{(Prop. 39).}$$

Lemma.—Every product of consecutive factors can be expressed as a sum of a product of lower consecutive factors.

$$\text{Thus } 1+4(n-4)+6\frac{(n-4)(n-5)}{1\,.\,2}+4\frac{(n-4)(n-5)(n-6)}{1\,.\,2\,.\,3}$$

$$+\frac{(n-4)(n-5)(n-6)(n-7)}{1\,.\,2\,.\,3\,.\,4}=\frac{n(n-1)(n-2)(n-3)}{1\,.\,2\,.\,3\,.\,4},$$

as appears by the equating coefficients of x^4 in the identity

$$(1+x)^{n-4}(x+1)^4=(1+x)^n.$$

Hence when $p=a$ in § 12,

$$(1+1)^m+2\frac{m(m-1)}{1\,.\,2}(1+1)^{m-2}+\frac{4\,.\,3}{1\,.\,2}\frac{m(m-1)(m-2)(m-3)}{1\,.\,2\,.\,3\,.\,4}(1+1)^{m-4}+\dots$$

$$=1+m\{1+(m-1)\}+\frac{m(m-1)}{1\,.\,2}\left\{1+2(m-2)+\frac{(m-2)(m-3)}{1\,.\,2}\right\}+\dots$$

$$=1^2+\left(\frac{m}{1}\right)^2+\left\{\frac{m(m-1)}{1\,.\,2}\right\}^2+\dots=\frac{1\,.\,3\dots(2m-1)}{1\,.\,2\dots m}2^m.$$

14. Dr. Stewart's general theorem (Prob. 42) follows from the same formula as the former.

II. *On Theorems relating to the Regular Polyhedra, which are analogous to those of Dr. Matthew Stewart on the Regular Polygons.*

1. These two general propositions may be thus stated in the dual form :—

(A.) Let there be a regular polyhedron of (n) faces, inscribed in a sphere of radius (a). If from the summits and centre there be drawn $p_1, p_2, \ldots p_n, p$ perpendiculars on any plane (exterior to the solid, if (m) is odd), the sum of the (m)th powers of the perpendiculars from the summits is a function of the perpendicular from the centre.

$$\Sigma(p_r)^m = \frac{n}{2(m+1)a}\{(p+a)^{m+1} - (p-a)^{m+1}\}.$$

This formula is applicable to all five Platonic bodies, if m be 1, 2, 3; if m be 4, 5, and not larger, it is restricted to the dodecahedron and icosahedron.

(B.) Under the same conditions as in (A), if there be taken any point, whose distance from the centre is (v), the sum of the ($2m$)th powers of the distances of this point from all the summits will be a function of its distance from the centre.

$$\Sigma(d_r)^{2m} = \frac{n}{2(m+1)av}\{(v+a)^{2m+2} - (v-a)^{2m+2}\}.$$

2. Following the analogy of plane geometry, I propose to consider a group of five surfaces, whose orthogonal projections are the tricuspid and quadricuspid hypocycloids, and which have the property, that the product of the perpendiculars drawn on any tangent plane from all the summits of one of three regular polyhedra (which are cuspidal points on those surfaces), is a function of that perpendicular only which is drawn on the same tangent plane from the centre of the sphere circumscribed about the polyhedron.

These three surfaces are defined by tangential polyhedral coordinates referred to the three first of the regular polyhedra.

$$(1)\quad p_1p_2p_3p_4 = p^4.$$
$$(2)\quad p_1p_2 \ldots p_6 = p^6 - a^2p^4.$$
$$(3)\quad p_1p_2 \ldots p_6 = p^6.$$
$$(4)\quad p_1p_2 \ldots p_8 = p^8 - \tfrac{4}{3}a^2p^6.$$
$$(5)\quad p_1p_2 \ldots p_8 = p^8.$$

3. By generalising the results of examination in each case of the regular polyhedra, it is found that the continued product of the perpendiculars drawn from all their summits on any plane may be thus expressed in terms of that drawn from the centre—

$$p_1p_2 \ldots p_n = p^n - \frac{1}{2\,.\,3}na^2p^{n-2} + \tfrac{1}{4}\left(\frac{n^2}{2\,.\,3^2} - \frac{n}{5}\right)a^4p^{n-4} - \ldots$$

Subsequent terms would involve the inclinations of lines and planes. But the following scale is found to exist:—

$$1-(n-2)fz^2+(n-4)gz^2-(n-6)hz^4+\ldots$$
$$=(1-nfz^2+ngz^4-nhz^6+\ldots)(1+\tfrac{1}{3}z^2+\tfrac{1}{5}z^4+\tfrac{1}{7}z^6+\ldots)$$

where f, g, h are found to have the preceding values:

$$2f=\tfrac{1}{3} : 4g=\tfrac{1}{3}fn-\tfrac{1}{5}: \text{ and } 6h \text{ would be } \tfrac{1}{3}gn-\tfrac{1}{5}fn+\tfrac{1}{7}.$$

$$\left(z \text{ is written in brief for } \frac{a}{p}.\right)$$

4. For the continued product of the perpendiculars on a parallel plane—

$$(p_1+x)(p_2+x)\ldots(p_n+x)=(p+x)^n-\frac{1}{2\,.\,3}na^2(p+x)^{n-2}+\ldots\equiv\mathrm{U}.$$

The sums of the several powers of $p_1, p_2, \ldots$ are found by taking the logarithm, and differentiating on both sides.

$$\frac{n}{x}-\frac{1}{x^2}\Sigma_1+\frac{1}{x^2}\Sigma_2-\frac{1}{x^3}\Sigma_3+\ldots=\frac{1}{\mathrm{U}}\frac{d\mathrm{U}}{dx}$$
$$=\frac{n}{p+x}\left\{1+\tfrac{1}{3}a^2\frac{1}{(p+x)^2}+\tfrac{1}{5}a^4\frac{1}{(p+x)^4}+\ldots\right\}$$
$$=\frac{n}{x}\left\{1-\frac{p}{x}+\frac{p^2}{x^2}-\frac{p^3}{x^3}+\ldots\right\}$$
$$+\frac{na^2}{3x^3}\left\{1-\tfrac{3}{1}\frac{p}{x}+\frac{4\,.\,3}{1\,.\,2}\frac{p^2}{x^2}-\ldots\right\}$$
$$+\frac{na^4}{5x^5}\left\{1-\tfrac{5}{1}\frac{p}{x}+\frac{6\,.\,5}{1\,.\,2}\frac{p^2}{x^2}-\ldots\right\}+\ldots$$

By equating the coefficients of like powers of x,

$$\Sigma(p_r)^m=n\left\{p^m+\frac{m(m-1)}{1\,.\,2}a^2p^{m-2}+\frac{m(m-1)(m-2)(m-3)}{1\,.\,2\,.\,3\,.\,4}a^4p^{m-4}\right\}$$
$$=\frac{n}{2(m+1)a}\{(p+a)^{m+1}-(p-a)^{m-1}\},$$

where m is restricted not to exceed 5.

Thus is established the first proposition (A) of § 1.

5. Proposition (B) of § 2 is proved as in § 4.

The form, being universal, is equally applicable, when $p_1, p_2, \ldots$ are used to denote the constants in the expression for distances, such as—

$$\delta_r{}^2=a^2+v^2-2av\cos\chi.$$

Write in this form—

$$a^2+v^2 \text{ for } p, \quad 2av \text{ for } a, \quad \delta_1, \delta_2^2, \ldots \delta_n^2 \text{ for } p_1, p_2 \ldots p_n,$$

$$(\delta_1^2+x)(\delta_2^2+x) \ldots (\delta_n^2+x)+(a^2+v^2+x)^n-\frac{4}{2\,.\,3}na^2v^2(a^2+v^2+x)^{n-2} + \ldots$$

$$\Sigma(\delta_r)^{2m}=\frac{n}{4(m+1)av}\{(v+a)^{2m+2}-(v-a)^{2m+2}\}.$$

6. Discussion of the first surface of the group.

$$p_1p_2p_3p_4=p^4=\frac{1}{4^4}(p_1+p_2+p_3+p_4)^4.$$

It satisfies both the required conditions of § 2; and no other surface formed from the regular tetrahedron satisfies the tests. Its quadriplanar equivalent in point-coordinates is of the tenth order.

The orthogonal projection on any face from its quadrantal pole, whose equation is

$$p_4=\tfrac{1}{4}(p_1+p_2+p_3+p_4)=\tfrac{1}{3}(p_1+p_2+p_3),$$

gives the tricuspid hypocycloid (see § 4 of Memoir I)

$$p_1p_2p_3=\frac{1}{3^3}(p_1+p_2+p_3)^3=p^3.$$

$$b^2c^2-4ac^3-4b^3d+18abcd-27a^2d^2=0,$$

where

$$a=\alpha\beta\gamma\delta, \quad b=\beta\gamma\delta+\alpha\gamma\delta+\ldots, \quad c=\alpha\beta+\alpha\gamma+\ldots, \quad d=\alpha+\beta+\gamma+\delta.$$

To ascertain its form two sections have been taken, (1) by a face, (2) by a plane through an edge and a centre.

From (1), when $\delta=0$, $\quad b^2(c^2-4bd)=0,$

that is, $\quad \alpha^2\beta^2\gamma^2\{\sqrt{(\beta\gamma)}+\sqrt{(\gamma\alpha)}+\sqrt{(\alpha\beta)}\}=0.$

The first factors denote the three edges, which are conjugate double lines, the last a tricuspid hypocycloid.

(2) Let $\gamma=\delta$, or the surface be intersected by a plane AOB, which passes through the edge AB, and bisects the edge CD perpendicularly. This would give the sections of greatest and least curvature; another such section superimposed vertically would give a clear conception of the surface.

The surface consists of six lobes, which are arranged in pairs, each pair being touched by the same asymptotic cone.

The edges of the asymptotic tetrahedron are conjugate lines, as is also the great circle at infinity.

7. In the same way the other surfaces are discussed and exhibited. The property (B) of Memoir I has its analogues on this group of surfaces and their duals.

IX. "On the Critical Point of Mixed Gases." By GERRARD ANSDELL, F.C.S. Communicated by Professor JAMES DEWAR, M.A., F.R.S. Received June 8, 1882.

Having on two previous occasions communicated to the Society papers on the physical constants of liquid hydrochloric acid gas and liquid acetylene, under which head I include the coefficients of compression and expansion, the critical points, and the volumes and tensions of the saturated vapour, it naturally led up to what promised to be a long investigation into the similar constants of other gases; and, amongst other things, the behaviour of two or more gases in presence of each other, more particularly with regard to the alteration of the critical point, appeared to me of especial interest.

These experiments, which I commenced nearly two years ago, were unavoidably interrupted at the time, and I have only now been able to resume them.

This subject has latterly engaged the attention of many physicists and chemists, and, amongst others, both Andrews and Cailletet have examined to a certain extent the behaviour of gaseous mixtures, the former finding both the critical point and vapour-tension of carbonic acid considerably modified by the introduction of a small quantity of pure nitrogen, and the latter ("Compt. Rend.," 90, 210) noting the peculiar behaviour of carbonic acid with one-fifth its volume of air, the former appearing to mix completely with the latter at 130 atmospheres pressure and 5°·5 C., forming a homogeneous mixture. More recently Amagat ("Compt. Rend.," 89, 1879) and Roth ("Wiedemann," N.F., 2, 1880) have contributed exhaustive papers on the deviation of gases from Mariotte's law. Clausius and Van der Waals have introduced new formulæ for calculating the critical point, Winkelman (Berichte, N.F., 2, 1880), Hannay ("Proc. Roy. Soc.," vol. 33) and others have been examining the relation between the different states of matter, and Ramsay and Pawlewski have investigated the behaviour of different liquid compounds with regard to their critical points, &c.; the former took equal weights of pure benzene and ether, and found that the critical temperature and pressure of the mixture was just between those of the individual bodies, but as he evidently experimented with only the one mixture, his results do not bear much upon the present problem, for which these experiments were undertaken, namely, the variation of the critical points of different percentage mixtures of two or more gases. Pawlewski's results, which seem to have an important bearing on the subject, I shall refer to more fully afterwards.

Before selecting any particular gases for investigation, there were several important points to be considered. In the first place, it was advisable to select those gases which could not only be easily prepared, but whose physical constants had been thoroughly investigated; it would be an advantage to use gases having comparatively low critical points, as the temperature would be much more easily kept constant, and would consequently contribute to the accuracy of the results; but above all those gases should be chosen which would not be likely to react upon each other in a liquid state, or at high temperatures and pressures, for this would modify the results considerably, from the probable formation of new compounds, &c. That this is likely to occur is shown in Professor Dewar's experiments on the behaviour of carbonic acid in presence of other bodies ("Proc. Roy. Soc.," 1880), where the carbonic acid often appeared to exist in a liquid state far above its critical point. This was no doubt due to the formation of a new compound; at least, as it could not be pure carbonic acid, we can only regard it as a compound of some kind formed under particular conditions of temperature and pressure, and this supposition seems to be confirmed by the experiment of carbonic acid in presence of camphor, where the camphor undoubtedly formed a new body, for we know how readily it combines with numerous substances such as hydrochloric acid, &c., to form unstable compounds. For these reasons I chose carbonic and hydrochloric acid gases, as they could be easily prepared, and their critical points had been very accurately determined, the former by Andrews, the latter by myself, besides being bodies most unlikely to be decomposed in each other's presence, more especially as they are chemically saturated bodies, and therefore according to the new chemical theory, most unlikely to form any addition or molecular compound.

The following method was adopted in the experiments, the Cailletet pump being used as described in my former papers. The carbonic acid was made by dropping pure strong sulphuric acid into a saturated solution of potash bicarbonate, being afterwards washed with distilled water, and dried by passing through four U-tubes with pounded glass and sulphuric acid. To check the readings of the air manometer (which was the same used in my former experiments) and also the purity of the gas, a tube was filled with the pure gas alone, and the tensions at different temperatures and the critical point were found to agree very well with Andrews' results. The hydrochloric acid gas was prepared by the action of strong sulphuric acid on pure chloride of ammonium, as described in my last paper ("Proc. Roy. Soc.," vol. 30), and was washed and dried with the usual precautions; a tube was also filled with the pure gas to begin with, and the critical point and tensions of the saturated vapour agreed as nearly as possible with those obtained by myself two years ago.

The purity of the individual gases and the accuracy of the air manometer having thus been proved, an ordinary Cailletet tube was chosen, having a capillary part about 2 millims. in diameter, and a total capacity of about 50 cub. centims. This was accurately calibrated, and then filled with the hydrochloric acid gas, by passing it through in a regular stream for about four or five hours; after sealing off, the bent end was placed under pure dry mercury, under the receiver of an air-pump, and a sufficient quantity of the gas withdrawn to make room for any amount of carbonic acid gas required to be introduced; the introduction was effected by passing a very fine capillary tube, bent at a particular angle and through which pure carbonic acid was streaming, round the bend of the tube while it remained under mercury, great care being taken to prevent the slightest trace of air from getting in. When sufficient carbonic acid had been introduced the tube was transferred to one of the iron bottles containing pure dry mercury, which was connected with another iron bottle containing the air manometer, and with the pump in the usual way.

The critical point of the mixture was first determined, and then the tensions of the saturated vapour at different temperatures, together with the fractional volume to which the gas was reduced at the point of liquefaction, and also the relation between the liquid and gaseous volumes at different heights in the tube.

At the end of the experiments the tube was carefully lifted out of the bottle, the outside of it well cleaned and dried with bibulous paper, and the end of it placed under distilled water. The small quantity of mercury in the bend of the tube was shaken out, and the water allowed to rush up the tube and absorb the hydrochloric acid gas; the solution was afterwards made up to 500 cub. centims. with distilled water, and 50 cub. centims. titrated with standard nitrate of silver, which gave the quantity of chlorine equal to the amount of hydrochloric acid in the tube. The small residue of mercury was dried and weighed, and the space it occupied subtracted from the total capacity of the tube, the remainder, after correction for temperature and pressure, being of course the volume of the mixed gases; from this was subtracted the volume of the hydrochloric acid gas calculated from the amount of chlorine obtained by titration, the remainder being carbonic acid, with, of course, any slight impurity of air or other inert gas that might be present.

The following tables give the tensions of the saturated vapour, at different temperatures, of the different percentage mixtures of pure hydrochloric acid and carbonic acid gases. They are also plotted in the form of curves on Plate I.

	I.		II.		III.		IV.	
	T.	P.	T.	P.	T.	P.	T.	P.
	0	27·84	0	28·86	0	33·17	0	31·89
	15	40·66	13·8	39·86	16·3	50·09	19·0	51·93
	27	54·22	25·5	52·77	25·4	63·98	25·6	60·46
	37·5	70·28	38·0	67·36	34·0	77·02		
	46	82·26	44·0	76·23	43·2	90·03		
C.P.=	47·2	92·21	45·5	80·52	45·1		39·5	80·28

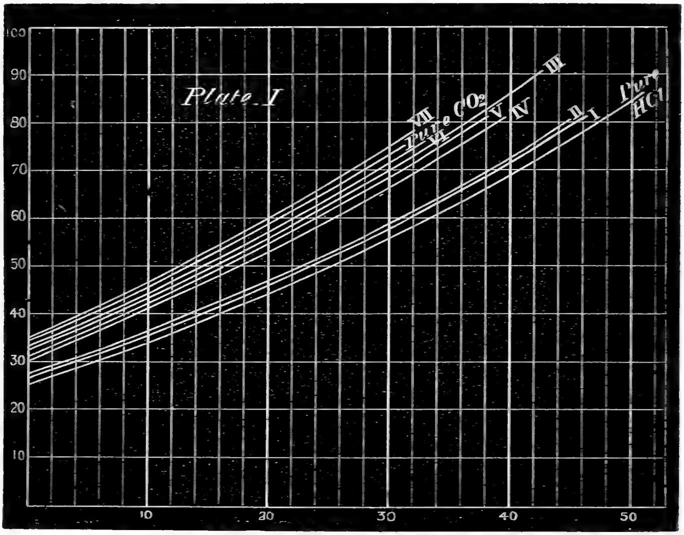
	V.		VI.		VII.	
	T.	P.	T.	P.	T.	P.
	0	32·72	0	34·56	0	34·65
	17·5	50·73	18·8	55·79	18·8	56·44
	26·6	63·31	25·5	65·68	24·9	67·27
	35·0	76·64				
	37·6	79·14				
C.P.=	38·0	81·35	33·5	77·69	32·4	77·23

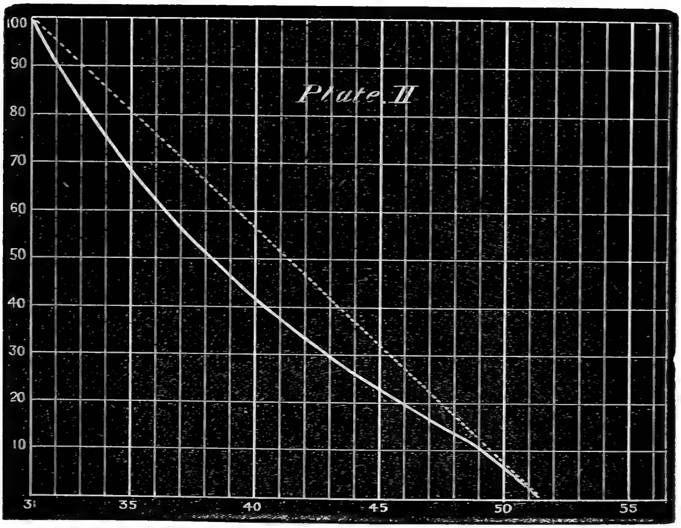
I=mixture containing 17·18 per cent. CO_2.
II= ,, ,, 19·37 ,, ,,
III= ,, ,, 25·48 ,, ,,
IV= ,, ,, 42·44 ,, ,,
V= ,, ,, 45·67 ,, ,,
VI= ,, ,, 74·18 ,, ,,
VII= ,, ,, 82·14 ,, ,,

T=temperature of mixed gases.
P=pressure in atmospheres.
C. P=critical point.

The critical points of the different mixtures are also plotted as a curve on Plate II. where the ordinates represent the percentage amount of carbonic acid in the mixture, and the abscissæ represent the temperature in degrees (centigrade). Now Pawlewski, in a short abstract of a paper ("Berichte," No. 4, 1882), describes a number of experiments he had made with the isomeric ethers, the alcohols, &c., and gives an equation to represent the critical point of mixtures of two or more liquids belonging to the same class of organic bodies, in terms of their respective critical points and relative weights, from which it would appear that the critical point of mixed bodies is directly proportional to the percentage composition of the mixture, when the origin of temperature taken is that of the body having the lowest critical point. He also mentions that this would probably be the case with the liquid form of substances which are gaseous at ordinary temperatures; but from our knowledge of liquefied gases, their physical constants are so much exaggerated with regard to their compression and expansion, &c.,

and the variation of their critical points is so much affected by small quantities of impurity, that we might naturally suppose gases having low critical points would not altogether follow this law, and the results of my experiments seem to confirm this view. It will be seen from the diagram, Plate II, that instead of descending in a straight line as it ought to do, according to Pawlewski's formula, it forms a





very regular curve, all the values being below those of Pawlewski's, until a point is reached where the mixture contains about 17 per cent. of carbonic acid, within which limit it evidently approaches to Pawlewski's values. The reason for this apparent anomaly can only be explained by the assumption that a small trace of air or other impurity was present in the tube, for we know from Andrews' and my own experiments, that even the $\frac{1}{500}$th part of air makes a considerable difference both in the critical point and tension of a gas.

It is also conceivable that a trace of air may have more effect in a mixture of two gases, than upon either individually, and this would consequently complicate matters considerably, when a mixture of several different gases is used. It was principally on this account that tensions of the saturated vapour of the mixture at different temperatures was taken, so as to judge of the amount of impurity in the gases, and whether it materially affected the results.

The curves on Plate I represent these tensions, the ordinates being the pressure in atmospheres, and the abscissæ the temperature in degrees C. The corresponding curves for pure hydrochloric acid and carbonic acid are also shown, but although all the curves for the different mixtures, except one, fall between the two, still the distances are evidently not strictly proportional to the percentage composition, which can only be explained by the presence of a small quantity of air; now, as this impurity must have been infinitesimal, it is interesting and curious to see how much it has modified the tensions of the saturated vapour. No. III should, of course, have come between No. II and No. IV, and must have had rather a larger amount of impurity than the others, and this is also the case in No. VII, which actually shows a tension higher than that of the most volatile constituent of the mixture, which, of course, is unprecedented.

Having satisfied myself that these apparent anomalies were really due to impurity, I filled another tube with extreme care, which contained the same relative proportions of the gases, within ·2 of a per cent. as No. VII, and found that it now assumed its proper place below the curve for carbonic acid, the critical point, however, being scarcely altered at all, showing that an amount of impurity, sufficient to materially modify the tensions of the vapour, had very little effect on the critical point.

The present position of the question therefore appears to be, that the critical points of mixtures of liquefied gases cannot be expressed by Pawlewski's formula, the maximum difference between his calculated value and mine, which occurred in a mixture of equal volumes of the gases, being as much as 3°·6 C. This is ·2 or 25 per cent. of the whole difference between the critical points of the two gases, hydrochloric acid being 51°·25, and carbonic acid 31° C.

But although these experiments seem to lead to this conclusion, more extended researches with other gases of the same nature, using the same precautions, will have to be made, before the real form of the curve can be ascertained.

This investigation has been carried out in the Laboratory of the Royal Institution.

X. "On an Arrangement of the Electric Arc for the Study of the Radiation of Vapours, together with Preliminary Results." By G. D. LIVEING, M.A., F.R.S., Professor of Chemistry, and J. DEWAR, M.A., F.R.S., Jacksonian Professor, University of Cambridge. Received June 8, 1882.

In previous papers* we have described various devices for facilitating the study of the reversal of the lines of metallic vapour. The first series of observations were made by examining the spectrum of the interior of iron or porcelain tubes filled with vapour and heated to the highest temperature of a coke furnace, the subsequent series being eye or photographic records of the radiation of the electric arc surrounded by metallic vapour in the middle of blocks or tubes of lime or magnesia.

By inclosing the arc in a crucible of lime or magnesia we have found its steadiness very greatly increased, and the mass of metallic vapour which can be maintained at a temperature approaching to that of the arc much enlarged, but it cannot be said that that temperature is at all under control, and the walls of the crucible are almost always cooler than the contents. By the arrangement we have now to describe we are able to make observations through a long range of temperature, as the temperature rises and as it falls, and so to trace the influence of temperature in many cases in which the extent of that influence was before doubtful. The temperature attainable is doubtless far below that of the arc, but still it is quite sufficient to maintain iron and aluminium in the state of vapour, and show the reversal of the lines of these elements with singular sharpness. The temperature of the interior is sufficiently high to transform the diamond into coke, even in a current of hydrogen, and the result may be taken as proving that the temperature is above that of the oxyhydrogen flame.

The apparatus employed is thus constructed: A rod of carbon, *a* in the figure, 15 millims. in diameter, perforated down its axis with a cylindrical hole 4 millims. in diameter, is passed through a hole in a lime block *d*, and is connected by means of a copper clip with the positive electrode of a Siemens dynamo-electric machine; another carbon rod *b*, unperforated, is passed into the lime block through a second hole at right angles to the first, so that the end of the rod *b* meets the rod *a* in the middle of the block of lime. The rod *b* is connected with the negative electrode of the dynamo machine, and after contact is made between the two carbons is raised a little so that the arc discharge continues between the two carbon rods within the block of lime or magnesia. In this way the outside of the rod or tube, *a*, becomes intensely heated, the heat is retained by the jacket of lime, and the interior of the tube gradually rises in the central part to a very high temperature. By stopping the arc it can be made to pass through the same stages of temperature in the inverse order. Observations are made by looking down the perforation. When the light issuing from the tube is projected by a lens on to the slit of a spectroscope, the heated walls of the tube give at top and bottom a continuous spectrum, against which various metallic lines are seen reversed, while in the central part, when the tube is open at the farther end, the spectrum is discontinuous, and the metallic lines seen reversed against the walls at top and bottom, appear as bright lines.

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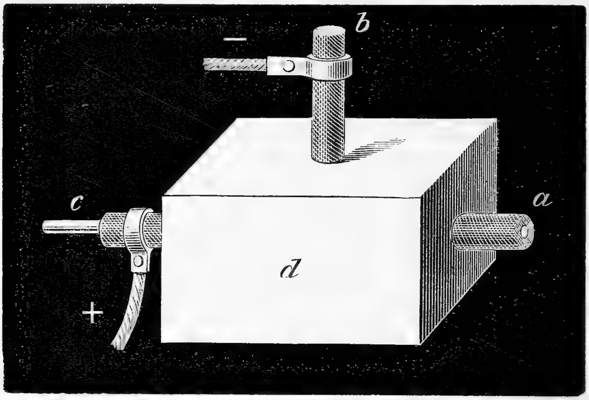
* "Proc. Roy. Soc.," "On the Reversal of the Lines of Metallic Vapours," vols. 28, 29, 32.



By passing a small rod of carbon *c* into the perforation from the farther end, a luminous background can be obtained all across the field, and then, as the walls of the tube are hotter than the metallic vapours between them and the eye, the metallic lines are only seen reversed. A very slight alteration in the position of the carbon rod makes the lines disappear, or reappear, or show reversal, and as the core is adjusted by eye observation before photographs are taken, all the conditions of the experiments are thoroughly known and are under easy control. We have taken photographs of the violet and lower part of the ultra-violet spectrum given by the tube at successive intervals while the temperature was rising, and noted the following results. When commercial carbons were used the first lines to be seen as the temperature rose were the potassium lines, wave-length 4044–6, next the two aluminium lines between H and K became conspicuous, then the manganese triplet about wave-length 4034, and the calcium line, wave-length 4226, then the calcium lines near M and an iron line, probably M, between them, and then gradually a multitude of lines which seem to be all the conspicuous iron lines between O and *h*. At this stage, when the small rod *c* is used to give a background, the bright continuous spectrum is crossed by a multitude of sharp dark lines, vividly recalling the general appearance of the solar spectrum. In the higher region the continuous spectrum extends beyond the solar spectrum, and the magnesium line, wave-length 2852, is a diffuse dark band, while all the strong iron lines about T, and the aluminium pair near S, are seen as dark lines. The behaviour of the calcium lines H and K is peculiar. These lines are often absent altogether, when the line wave-length 4226 and the two near M are well seen, and when the two aluminium lines between them and many of the iron lines are sharply reversed. Even the introduction of a small quantity of metallic calcium or calcium chloride into the tube did not bring them out reversed. They were only seen as bright lines, not very strong, when the small rod *c* was removed.

In some of the photographs H is visible as a bright line without K. We have formerly observed that K shows reversal in the electric arc spectrum taken in a lime crucible on the addition of aluminium, when H remains bright, and such a condition as that shown by the hollow carbon tube where H is present without K, might legitimately have been predicted. The lithium lines at 4603 and 4131 are often bright when many other lines in the neighbourhood are reversed, and must, therefore, be regarded as relatively difficult of reversal. As a rule the lines less refrangible than 4226 are balanced as to their emissive and absorptive power, and, therefore, disappear, while the more refrangible are reversed. The cyanogen group at 3883 remain bright when the iron lines on either side are reversed; they often, however, disappear on the continuous spectrum. Many lines about P and Q of the solar spectrum are reversed. The cyanogen band above K is generally to be found in the photographs of the spectrum when only air is in the tube. It is then very faint, and is the only cyanogen group visible. If ammonia is passed into the tube the fine set above K, the N group, and, although less plainly marked, the set at 4218, appear. In one plate the three lines at 4380 and the group of seven at 4600 appear along with the blue hydrocarbon set. It is well known that ammonia reacts on carbon at a white heat, producing cyanide of ammonium and hydrogen, so that the genesis of the cyanogen spectrum under the present conditions is a crucial test of the validity of our former observations on this subject, which are, however, in marked disagreement with the results obtained by Mr. Lockyer, in his review of the same field of investigation.

Both the indium lines 4101 and 4509 are persistently reversed, together with several lead lines. Tin gives flutings in highly refrangible portions of the spectrum, and silver gives a fine fluted-looking spectrum in the blue. Chloride of calcium gives a striking set of six or seven bands between L and M, which may be seen both bright and reversed.

When the small rod *c* is removed, it is easy at any moment to sweep out the vapours in the tube by blowing through it; it is equally easy to pass in reducing or other gases. Ammonia introduced seems to facilitate the appearance of reversed lines. On passing this gas through a tube containing magnesia, the set of lines just below *b*, which we have always found to be associated with the presence of magnesium and hydrogen, and is most probably due to some compound, instantly appear.

The above is a brief abstract of the few observations we have been able to make as a preliminary to a more thorough research, and we feel warranted in thinking that the method promises to solve some intricate spectroscopic problems. When we can command several electric arcs to heat a considerable length of carbon tube, and are enabled to examine the radiation of a powerful arc passing through vapours in the tube, valuable results may be anticipated.

XI. "On the Ultra-violet Spectra of the Elements. Part I. Iron." By G. D. Liveing, M.A., F.R.S., Professor of Chemistry, and J. Dewar, M.A., F.R.S., Jacksonian Professor, University of Cambridge. Received June 8, 1882.

(Abstract.)

By means of photographs taken with a Rutherford grating of 17,296 lines to the inch, the authors have determined the wave-lengths of ninety-one of the most prominent lines in the spark spectrum of iron between wave-lengths 2948, the termination of Cornu's map of the solar spectrum, and 2327, and also of fourteen of the strongest lines in the spark spectrum of copper beyond that up to the wave-length 2135. Using these lines as lines of reference they have, from photographs taken with calcite prisms, deduced the wave-lengths of 584 more lines in the arc and spark spectra of iron within those

limits. These lines are mapped on the same scale as Ångström's and Cornu's maps of the solar spectrum. The paper describes the method of taking the measures, and gives in detail the quantities observed and the data on which the calculations are founded.

Part II. Received June 15, 1882.

(Abstract.)

In the second part of this paper the authors have given a map of the ultra-violet lines of potassium, sodium, lithium, barium, strontium, calcium, zinc, mercury, gold, thallium, aluminium, lead, tin, antimony, bismuth, and carbon, as developed in the arc. They point out that in several cases the lines are in all probability harmonically related, as shown by the repetition of similar groups of lines at regularly diminishing distances, the groups being alternately sharply defined and diffuse, and becoming more diffuse as they die away at the end of the series. They had previously called attention to this kind of relationship between the visible lines in the spectra of the alkalies and of magnesium. The like relationship holds good in the ultra-violet spectra of those metals, and is strongly marked in the cases of calcium and zinc, less strongly in some other metals.

XII. "General Observations on the Spectra of Carbon and its Compounds." By Professor G. D. LIVEING, M.A., F.R.S., and Professor JAMES DEWAR, M.A., F.R.S. Received June 12, 1882.

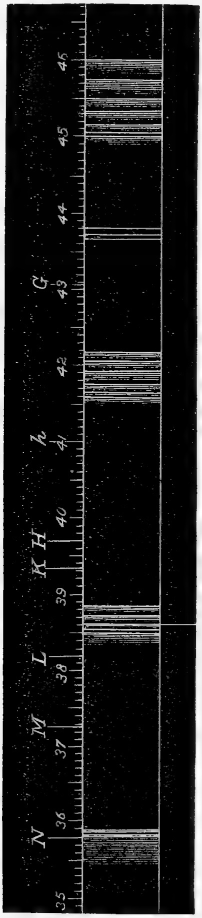
In our two former papers on the spectra of the compounds of carbon with hydrogen and nitrogen ("Proc. Roy. Soc.," vol. 30) we described the results of a long series of synthetical and analytical experiments which had enabled us to trace satisfactorily a fluted band spectrum which occurs in the arc and spark discharge in many compounds of carbon, and generally when carbon poles are used to transmit the current of the arc or spark in air, to the compound substance cyanogen. This led to a further investigation of the carbon ultra-violet line spectrum in order to complete the series of simple vibrations which originate from this substance. After all this work a great deal remains to be ascertained regarding the conditions which cause a variation of intensity in the different series of carbon flutings which originate from cyanogen, and also their persistency and development.

The present paper is a short record of the particular variations in the carbon groups which are revealed in the different photographs of the spectrum of the arc discharge that we have had occasion to take for other purposes, together with some new observations on the genesis of the cyanogen spectrum during combustion.

The remarkable discovery of Dr. Huggins, regarding the occurrence of two of the most marked series of cyanogen bands in last year's comet, adds considerable interest to this question, and has induced us to make a further study of the chemical reactions in flames which cause this particular spectrum to appear at a relatively low temperature.

Electric Discharge between Carbon Poles in different Gases.

In order to facilitate reference the general appearance of the portion of the cyanogen spectrum to which we shall refer is given in the following diagram :—



The apparatus used in the experiments has been already described in our paper on the spectrum of the compounds of carbon with hydrogen and nitrogen ("Proc. Roy. Soc.," vol. 30). No attempt was made to use perfectly pure gases or to remove all traces of nitrogen from the vessels employed; the object being to study the variation of the groups of lines, perfect purity in the gases was not required.

The arc discharge between graphite poles in carbonic acid shows the triple set, beginning about 4380, with traces of the other sets of cyanogen bands at 4218 and 3883. If the carbonic acid gas is displaced by air, the triple set are very much weakened and are sometimes invisible, while the other fluted series at 4218 and 3883 are greatly strengthened.

The spark discharge does not show the cyanogen sets in carbonic acid, but a series of five groups appear between the limits of the lines S and N of the solar spectrum, which may possibly be due to carbonic acid or carbonic oxide. The carbonic oxide flame, however, does not show this set. If the spark discharge is taken between graphite poles in nitrogen all the cyanogen series appear; but in hydrogen they sometimes disappear. As a rule, however, they remain faint even when a current of the gas is kept continuously passing through the bulb in which the discharge is taken. With the arc discharge in hydrogen the triple set are well marked, while the series at 4218 disappear, and the ultra-violet group are just visible; the hydrocarbon set, however, at 4310 come out strong.

In order to ascertain how the pressure of the surrounding atmosphere affected the emissive power of the cyanogen produced synthetically in the arc discharge, a series of observations on the spectra obtained under diminished pressure of the gaseous atmosphere was undertaken. The pressure in different gases was reduced to a mean value of about 1 inch of mercury, and under such conditions the intermittent discharge of the De Meritens machine was examined. The arc in air, under these circumstances, showed the blue hydrocarbon set; all the cyanogen series of bands together with a nitrogen series near H. Carbonic acid at the same pressure had the triple set of lines strongly marked, while the others were decidedly weaker, as in the experiments with the gas at atmospheric pressure.

In hydrogen, at equal pressure, the triple set disappear, and the hydrocarbon set at 4310 occur, which series is not generally seen in the photographs of the arc spectrum. But what is very remarkable is the appearance of two lines of carbon, viz., 2836·3 and 2837·2 (also 2506 and 2508) in the spectrum of the discharge, whereas these spark lines are not generally found in the arc spectrum.

The following lines of carbon have been observed in the arc discharge of the Siemens continuous current machine, as well as in that of the De Meritens intermittent current, taken in air. We have observed in previous experiments that the De Meritens arc in hydrogen produces a sufficient temperature to render the C line of hydrogen permanently visible. The continuous Siemens current, under the same circumstances, only shows this line when the arc is produced by breaking the contact of the poles, not with the steady arc.

Approximate Wave-lengths of Carbon Arc-lines.

2434·8 absent from spark.
2478·3 strongest line.
2506·6.
2514·1.
2515·8.
2518·8.
2523·9.
2528·1.
2881·1 not in spark.

We have here another instance of the lines of high refrangibility appearing, under certain circumstances, when no trace of strong lines belonging to the less refrangible portion of the spectrum can be detected. Thus the strong carbon line in the blue at 4266 does not appear in the photographs of the arc spectra. Of course it is possible that a very long exposure of the photographic plate might reveal some of the missing lines, as we have shown in other cases. The presence of these carbon lines is a proof that carbon vapour of a definite, but probably low, tension exists in the arc discharge, and this is doubtless the reason why under such conditions carbon combines with hydrogen and nitrogen with such facility. By a careful series of experiments carried out at different pressures with varying electric power we hope to ascertain with greater precision the variations in the carbon line spectrum.

Eye Observation of Spectra.

When the spectrum of different parts of a magnified image of the electric arc is examined, all the more refrangible cyanogen groups may be seen near the positive pole, together with a series of channellings in the red. When the arc is steady the cyanogen spectrum is permanently visible at the negative pole, when no trace of the hydrocarbon series can be seen. In the same way the arc in the middle of a magnesia crucible often shows no trace of the hydrocarbon set, although the cyanogen are strong. If, however, puffs of air or carbonic acid are passed into the arc, the hydrocarbon lines are produced. There is always, under these circumstances, far greater variation in the brightness of the hydrocarbon series than of the cyanogen, in fact, the presence of magnesia rather favours the steady formation of cyanogen. When the hydrocarbon spectrum is strong the brilliancy and number of the cyanogen groups that are visible are undoubtedly increased, so that the one variety of vibrations seems to affect the other. This is easily accounted for by the chemical interaction which takes place between acetylene, nitrogen, and hydrocyanic acid. The hydrocarbon spectrum is brought out at once in the magnesia crucibles by moistening one of the poles. All such actions seem to show that hydrogen is essentially connected with the production of this fluted spectrum just as nitrogen is with the cyanogen series.

Arc Discharge in Fluids.

The De Meritens arc, taken in water, shows the hydrocarbon spectrum alone; no cyanogen bands can be seen by eye observation, even when ammonia or nitrate of potash is added to the water. In this case the observations are rendered uncertain from the great intensity of the continuous spectrum. If glycerine is used instead of water no cyanogen groups of lines can be recognised, but on adding a little nitrobenzol the set of three lines (about 4380) peculiar to the cyanogen spectrum appear, this being the only group which can be detected by the eye on the continuous background. This result supports the observations on the varying intensity of this group in different gaseous media, and seems to show that conditions can be found where it is the most characteristic group of cyanogen. These three lines are easily seen in the spectrum of the arc taken in carbonic acid. although they disappear from the spectrum of the arc taken in air.

Vacuum Tube Spectra.

In our former experiments with vacuum tubes we did not use a capillary glass tube, but preferred to examine the photographic spectrum obtained from a short spark taken between platinum wires. Objection has been taken to this plan of working on the ground that, as the capillary form of vacuum tube increases the brilliancy of the spectrum, particular lines or groups of the spectrum which otherwise would be missed, might be revealed in them, and such tubes ought, therefore, to have been employed. In order to answer this objection we have prepared and examined vacuum tubes of this kind containing benzol and benzol with naphthalene in solution, using all the precautions to avoid the presence of nitrogen formerly described, and have always found such tubes free from any trace of the cyanogen spectrum. When such tubes are, however, examined daily, the cyanogen bands often appear after a time, and this can be traced in all such cases to a leak or crack at the point where the platinum is sealed into the glass. No perfectly pure hydrocarbon gives the series of bands we attribute to cyanogen.*

Observations on Flames.

The temperature produced by the combustion of hydrocarbons and other non-nitrogenous organic bodies well supplied with oxygen, is not sufficient to induce the combination of nitrogen with carbon, so that the cyanogen spectrum is absent from such flames. We know, however, that hydrocyanic acid is often produced in the oxidation of organic bodies containing nitrogen, and that ammonia reacts with carbon at a white heat, producing hydrocyanic acid and hydrogen. Such actions led us to expect that the cyanogen spectrum ought to appear in the flames of organic compounds containing nitrogen, provided the temperature were sufficient to render the radiation of this substance sufficiently intense. Our first experiments did not succeed. The most careful examination by the eye of the spectrum of a hydrogen flame which had passed through a solution of hydrocyanic acid or of a flame of alcohol containing nitrobenzol or nitrite of ethyl, did not result in any recognition of the strong cyanogen groups. This failure led to a chemical examination of the composition of the gases withdrawn from the interior of such flames, in order to ascertain the combustible mixtures which react during combustion to produce hydrocyanic acid. The gases were extracted from the flame with an apparatus similar in principle to that employed by Deville in his "Chemical Researches on Flame." When coal gas is passed through a solution of ammonia and burnt, the flame gases contain hydrocyanic acid and acetylene, but if oxygen is well supplied to the flame no cyanogen reaction is given by the extracted gas. Carbonic oxide mixed with ammonia in the same way gave no trace of hydrocyanic acid during combustion: even when a large quantity of the mixture was burnt and the flame gases continuously withdrawn no appreciable cyanogen reaction could be detected. Similarly hydrogen mixed with a little carbonic acid and ammonia gave no cyanogen reaction. When hydrogen is passed through ammonia solution mixed with chloroform, tetrachloride of carbon, bisulphide of carbon, or picoline, cyanogen can always be recognised in the flame gases. Chloroform under such circumstances yields the largest amount. When a mixture of carbonic oxide and ammonia is passed through a porcelain tube heated in a furnace, large quantities of hydrocyanic acid are produced, especially when the moist gases are employed. Ammonia passed over perfectly pure graphite at a white heat produces hydrocyanic acid, and the vapour of chloride of ammonium is equally efficient in bringing about this reaction. It appears to result from the experiments that hydrocyanic acid can always be separated from the interior of flames such as we have employed, provided that portion of the flame which, in carbon compounds, is characterised as reducing, be selected. That stage of combustion during which free carbon or dense hydrocarbon vapours containing very little hydrogen are formed, is favourable to the formation of hydrocyanic acid, as ammonia can at this stage react on the carbon. It is quite possible, however, that hydrocyanic acid may exist in small quantity in some of the flames which tested according to this method appear to contain none, and that notion is favoured by the consideration of the dissociation phenomena which are known to occur in flames. This led us again to spectroscopic examination as the most delicate test for the presence of cyanogen, but instead of trusting to the eye as in former experiments, photographs were taken of the spectra of flames by means of a quartz and calcspar train, and the exposure of the plate purposely prolonged. Thus examined, it was found that coal gas well supplied with oxygen gave only the hydrocarbon groups, together with the two interesting additional lines first discovered by Dr. Huggins, having the wave-lengths 3872 and 3890; but when the coal gas passed through ammonia the photographs revealed the characteristic cyanogen groups at 3883 and 4218, the most refrangible group being the strongest. The cyanogen spectrum can then be produced synthetically from nitrogen compounds in flames along with the hydrocarbon spectrum, so that the appearance of the groups of cyanogen is not always associated with a very high temperature such as we have in the electric arc. Cyanogen once formed gives its peculiar spectrum at the relatively low flame temperature produced by burning cyanogen mixed with carbonic acid. Of course the mean temperature of a flame is very different from the temperature of individual molecules, and this complicates the problem we are discussing. The thermal equivalents of cyanogen and acetylene being highly negative, it is certain that these substances yield on combustion the highest temperature of any two compounds burning in oxygen; and we have shown in a former paper that burning cyanogen in nitric oxide gas, which probably induces a still higher temperature, does not bring about any marked change in the character of the spectrum. Spectroscopic analysis can thus detect very small quantities of cyanogen under widely different physical conditions. As the temperature of the flame of cyanogen probably approaches the temperature of the carbon poles of the electric arc, and as we have shown that carbon undoubtedly exists in the form of vapour in the arc discharge, from the fact of the ultra-violet line spectrum being present, the question naturally arises, is carbon present in the form of vapour in the cyanogen flame? In order to answer this question we have taken photographs of the ultra-violet spectrum of the cyanogen flame fed with oxygen, and with long exposures have had no difficulty in detecting one of the strongest carbon lines, viz., that at 2478·3, along with a trace of what may be the pair of lines at 2837, but more probably is a mercury line. No other carbon line was found in the photographs. It seems, therefore, proved that carbon vapour does exist in the flame of cyanogen, although to a much smaller extent than in the arc discharge. Observations must be made on the spectra of flames under high pressures, in order to solve many problems connected with spectroscopic enquiry, and this subject we hope to discuss in a future communication.

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* It is worthy of note that the strong carbon line wave-length 2478·3 present in both the arc discharge and in the spark discharge in carbon compounds at atmospheric pressure, is not found in the spectrum of the spark in cyanogen at low pressure. We have tried to obtain a photograph of it from a "Plücker" tube fitted with a quartz end, and placed end-on in front of the spectroscope, but found no trace of it. As this line appears in the spectrum of the flame of cyanogen, its absence from the spark discharge in cyanogen of low tension seems intelligible only on the supposition that the discharge is selective in its course, and lights up only certain of the substances present, or else that the quantity of carbon vapour present at any instant is so minute, as to produce no sensible effect on the photographic plate.—July 10, 1882.

XIII. "Further Observations upon Liquid Jets, in continuation of those recorded in the Royal Society's 'Proceedings' for March and May, 1879." By Lord RAYLEIGH, F.R.S., Professor of Experimental Physics in the University of Cambridge. Received June 8, 1882.

The experiments herein described were made in the spring and summer of 1880, with the assistance of Mrs. Sidgwick. Section 2 was indeed written out as it now stands in August of that year. There were some other points which I had hoped to submit to examination, but hitherto opportunity has not been found.

On some of the Circumstances which influence the Scattering of a nearly Vertical Jet of Liquid.

§ 1. It has been already shown that the normal scattering of a nearly vertical jet is due to the rebound of the drops when they come into collision. If, by any means, the drops can be caused to amalgamate at collision, the appearance of the jet is completely transformed. This result occurs if a feebly electrified body be held near the place of resolution into drops, and it was also observed to follow the addition of a small quantity of soap to the water of which the jet was composed. In trying to repeat the latter experiment in May, 1880, at Cambridge, I was astonished to find that even large additions of soap failed to prevent the scattering. Thinking that the difference might be connected with the hardness of the Cambridge water—at home I had used rain water—I repeated the observations with distilled water, but without finding any explanation. The jet of distilled water scattered freely, both with and without soap, and could only be prevented from doing so by electricity. Eventually the anomalies were traced to differences in the character of the soap. That used at Cambridge up to this point was a clarified specimen prepared for toilet use. On substitution for it of common yellow soap, the old effects were fully reproduced.

Further experiment seemed to prove that the real agent was not soluble soap at all. If water impregnated with the yellow soap was allowed to stand, a white deposit separated, after which the supernatant liquid was found to be inactive. But after shaking up the same effects were produced as at first. The addition of caustic potash to the unclarified soapy mixture destroyed its power. On the other hand, sulphuric acid rendered the clarified soap solution active.

The natural conclusion from these facts would be that the real agent is unsaponified greasy matter distributed through the liquid; and this view is confirmed by the striking results which follow the addition of small quantities of milk. The experiment may be made conveniently by connecting a Woulf's bottle with the water tap by a rubber tube fitted to one tubulure, while the vertical nozzle is in connexion with another tubulure. If a little milk be placed in the bottle, the jet of opalescent liquid apparently coheres, and passes the summit in one unbroken stream. After a time the milk is gradually washed out, and the scattering is re-established. About one drop of skimmed milk per ounce of water is sufficient to produce the effect.

I must not omit to mention that on several occasions distinct evidence was obtained that it is possible for soap to be in excess. With a large quantity the coherence of the jet was imperfect, and was improved by dilution. The complete elucidation of the subject probably requires more chemical knowledge and experience than is at my command.

Of the various other substances which have been tried, such as glycerine, sugar, gum arabic, alcohol, sulphuric acid, none have been found active.

Vertical fountains of mercury were found not to scatter. The head was about 15 inches, and various glass nozzles were used from $\frac{1}{20}$ inch to $\frac{1}{50}$ inch in diameter. Also a nozzle terminating in an amalgamated brass plate, through which a hole of $\frac{1}{20}$ inch was pierced. In all these cases the drops of mercury coalesced at collision, behaving in the same way as drops of milky water issuing from the same nozzles. Fountains of clean water issuing from these nozzles under the same pressure scattered freely.

When the diameter of the nozzle from which a water jet issues is reduced to below $\frac{1}{100}$ inch, the scattering cannot be completely prevented by the presentation of an electrified body. One possible reason for this is evident. The mutual repulsion of the similarly electrified drops increases rapidly relatively to the masses as the size is reduced, and thus it may happen that before the *differential* electrification sufficient to rupture the separating envelope at contact is arrived at, the repulsion may be powerful enough to prevent most of the drops from coming into contact at all. In connexion with this it may be remarked that two perfectly equal and equally electrified spheres would repel one another at all distances; but that if there be the slightest difference in the size or electrification, the repulsion will be exchanged for attraction before actual contact is attained. This attraction will be local, and thus the opposed parts of the surfaces may come into contact with considerable violence, even when the relative motion of the centres of the masses is small. It is easily shown experimentally (see § 4) that violence of contact tends to promote coalescence, so that we have here a possible explanation of the action of electricity.

With respect to the persistent scattering of very fine jets, however, it would appear that the principal cause is simply that many of the fine drops fail to come into contact in any case. The capillary forces act with exaggerated power, and doubtless impress upon the minute drops irregular lateral velocities, which may easily reach a magnitude sufficient to cause them to clear one another as they pass. At any rate little difference is observable in this respect between a fine jet of clean water under feeble electrical influence, and one to which a little milk has been added, but without electrification.

With a suitable jet, say from a nozzle about $\frac{1}{20}$ inch diameter, and rising about 2 feet, the sensitiveness to electricity is wonderful, more especially when we remember that the effect is differential. I have often caused a jet to appear coherent, by holding near the place of resolution a brass ball about 1 inch in diameter, supported by a silk thread, and charged so feebly that a delicate gold-leaf electroscope would show nothing. Indeed, some care is necessary to avoid being misled by accidental electrifications. On one occasion the approach of a person, who had not purposely being doing anything electrical, invariably caused a transformation in the appearance of the jet.

The jets hitherto under discussion are such as resolve themselves naturally into drops soon after leaving the nozzle, or at any rate before approaching the summit of their path. If the diameter be increased, we may arrive at a condition of things in which the undisturbed jet passes the summit unbroken. In such a case the addition of milk, or the presentation of an electrified body, produces no special effect. One interesting observation, however, may be made. By the action of a vibrator of suitable pitch, *e.g.*, a tuning-fork, resolution on the upward path may be effected. As the vibration gradually dies down, the place of resolution moves upwards, but it cannot pass a certain point. When the point is reached, resolution into actual drops ceases, the upper part of the jet exhibiting simple undulations, when viewed intermittently. The phenomenon is in perfect harmony with theory. As it leaves the nozzle, the jet is unstable for the kind of disturbance imposed upon it by the vibrator. The subsequent loss of velocity, however, shortens the wave-lengths of disturbance, until at length they are less than the circumference of the jet, after which the disturbance changes its character from unstable to stable. The vibrator must evidently produce its effect quickly, or not at all.

Influence of Regular Vibrations of Low Pitch.

§ 2. Towards the close of my former paper on the capillary phenomena of jets, I hazarded the suggestion that the double stream obtained when an obliquely ascending jet is subjected to the influence of a vibration, an octave graver than the natural note, is due to the *compound* character of the vibration. At the time of Plateau's researches the fact that most musical notes are physically composite was much less appreciated than at present, and it is not surprising that this point escaped attention. I have lately repeated Plateau's experiments under improved conditions, with results confirmatory of the view that no adequate explanation of the phenomena can be given which does not have regard to the possible presence of overtones; and I have added some observations on the effects of the simultaneous action of two notes forming a consonant chord.

In order to make a satisfactory examination of it, it is necessary to employ some apparatus capable of affording an intermittent view of the jet in its various stages of transformation. In the experiments formerly described I used sparks from an induction coil, governed by the same tuning-fork which determined the resolution of the jet. This has latterly been replaced by a perforated disk of black cardboard, driven at a uniform speed by a small water-motor. The diameter of the holes is one-fifth of an inch—about that of the pupil of the eye, and the interval between the holes is about four inches. Examined under these conditions the jet and resultant drops are sufficiently well defined, and there is abundant illumination if the apparatus is so arranged that the jet is seen projected against the sky. The speed of the motor is regulated so that there is one view through the holes in about one complete period of the phenomenon to be observed. If the power is a little in excess, the application of a slight friction to the axle carrying the disk renders the image steady, or, what is better, allows it to go forwards through its phases with moderate slowness.

Although the multiple streams are better separated when the jet is originally directed upwards at an angle of about 45°, I preferred to use a horizontal direction as giving simpler conditions. The velocity and diameter are then practically constant throughout the transformation, and may be readily calculated from observations of the head and of the total quantity of fluid discharged in a given time. The reservoir consisted of a large glass bottle, provided with a tubulure near the bottom. Into this was fitted a 1-inch brass tube, closed at the end by a flat plate, in which a circular aperture was pierced of about $\frac{1}{12}$ of an inch in diameter.

If h=head,
d=diameter of *jet*,
v=velocity of issue,
V=volume discharged in unit time,

then

$$\tfrac{1}{4}\pi d^2 v = \mathrm{V}, \qquad v = \sqrt{(2gh)}.$$

Again, if N′ be the frequency of the most rapid vibration which can influence the jet, we have by Plateau's theory—

$$\mathrm{N}' = \frac{v}{\pi d} = \frac{v^{\frac{3}{2}}}{2\sqrt{(\pi \mathrm{V})}} = \frac{(2gh)^{\frac{3}{4}}}{2\sqrt{(\pi \mathrm{V})}}.$$

If N be the frequency of the principal note of the jet, then, as explained in my former paper,

$$\mathrm{N} = \frac{3{\cdot}142}{4{\cdot}508}\mathrm{N}'.$$

In the present experiment it was found that 1050 cub. centims. were discharged in four minutes, and the head was $7\frac{3}{4}$ inches, so that in C.G.S. measure—

$$\mathrm{V} = \frac{1050}{240}; \qquad h = 7\tfrac{3}{4} \times 2{\cdot}54; \qquad g = 981;$$

whence

$$\mathrm{N}' = 372, \qquad \mathrm{N} = 259.$$

As sources of sound tuning-forks, provided with adjustible sliding pieces, were employed, except when it was important to eliminate the octave as far as possible; the vibration was communicated to the reservoir through the table on which it stood. The forks were either screwed to the table and vibrated with a bow, or mounted on stands (resting on the table) and maintained electrically. The former method was quite adequate when only one fork was wanted at a time.

With pitches ranging from 370 to about 180, the observed phenomena agreed perfectly with the unambiguous predictions of theory. From the point—decidedly below 370—at which a regular effect was first obtained, there was always one drop for each complete vibration of the fork, and a single stream, every drop breaking away under the same conditions as its predecessor. After passing 180 it becomes a question whether the octave of the fork's note may not produce an effect as well as the prime. If this effect be sufficient the number of drops is doubled, and unless the prime be very subordinate indeed, there is a double stream, alternate drops taking sensibly different courses. In these experiments the influence of the prime was usually sufficient to determine the number of drops, even in the neighbourhood of pitch 128. Sometimes, however, the octave became predominant, and doubled the number of drops. It must be remembered that the relative intensities with which the two vibrations reach the jet depend upon many accidental circumstances. The table has natural notes of its own, and even the moving of a weight upon it may change the conditions very materially. When the octave is not strong enough actually to double the drops, it often produces an effect which is very apparent to an observer examining the transformation through the revolving holes. On one occasion a vigorous bowing of the fork which favours the octave, gave at first a double stream, but this after a few seconds passed into a single one. Near the point of resolution those consecutive drops which ultimately coalesce as the fork dies down, are connected by a ligament. If the octave is strong enough this ligament breaks, and the drops are separated, otherwise the ligament draws the half-formed drops together, and the stream becomes single. The transition from the one state of things to the other could be watched with facility.

In order to get rid entirely of the influence of the octave a different arrangement is necessary. It was found that the desired result could be arrived at by holding a 128 fork in the hand over a resonator of the same pitch resting on the table. The transformation was now quite similar in character to that effected by a fork of frequency 256, the only differences being that the drops were bigger and twice as widely spaced, and that the *spherule*, which results from the gathering together of the ligament, was much larger. We may conclude that the cause of the doubling of a jet by the sub-octave of the note natural to it is to be found in the presence of the second component, from which scarcely any musical notes are free.

When two forks of pitches 128 and 256 were sounded together, the single or double stream could be obtained at pleasure by varying the relative intensities. Any imperfection in the tuning is rendered very evident by the behaviour of the jet, which performs evolutions synchronous with the audible beats. This observation, which does not require the aid of the revolving disk, suggests that the effect depends in some degree upon the relative phases of the two tones, as might be expected *à priori*. In some cases the influence of the sub-octave is shown more in making the alternate drops unequal in magnitude, than in projecting them into very different paths.

Returning now to the case of a single fork screwed to the table, it was found that as the pitch was lowered below 128, the double stream was regularly established. The action of the *twelfth* below the principal note ($85\frac{1}{3}$) demands special attention. At this pitch we might in general expect the first three components of a compound note to influence the result. If the third component were pretty strong it would determine the number of drops, and the result would be a threefold stream. In the case of a fork screwed to the table the third component of the note must be extremely weak, if not altogether missing; but the second (octave) component is fairly strong, and in fact determines the number of drops ($190\frac{2}{3}$). At the same time the influence of the prime ($85\frac{1}{3}$) is sufficient to cause the alternate drops to pursue different paths, so that a double stream is observed.

By the addition of a 256 fork there was no difficulty in obtaining the triple stream, but it was of more interest to examine whether it were possible to reduce the double stream to a single one with only $85\frac{1}{3}$ drops per second. In order to secure as strong and as pure a fundamental tone as possible, I cause it to act in the most favourable manner upon the jet, the air space over the water in the reservoir was tuned to the note of the fork by sliding a piece of glass over the neck so as partially to cover it. When the fork was held over the resonator thus formed, the pressure which expels the jet was rendered variable with a frequency of $85\frac{1}{3}$, and overtones were excluded as far as possible. To the unaided eye, however, the jet still appeared double, though on more attentive examination one set of drops was seen to be decidedly smaller than the other. With the revolving disk, giving about eighty-five views per second, the real state of the case was made clear. The smaller drops were the *spherules*, and the stream was single in the same sense as the streams given by pure tones of frequencies 128 and 256. The increased size of the spherule is of course to be attributed to the greater length of the ligament, the principal drops being now three times as widely spaced as when the jet is under the influence of the 256 fork.

With still graver forks screwed to the table the number of drops continued to correspond to the second component of the note. The double octave of the principal note (64) gave 128 drops per second, and the influence of the prime was so feeble that the duplicity of the stream was only just recognisable. Below 64 the observations were not carried. Attempts to get a single stream of 64 drops per second were unsuccessful, but it is probably quite possible to do so with vibrations of greater power than I could command.

In the case of a compound note of pitch 64 a considerable variety of effects might ensue, according to the relative strengths of the various components. Thus, the stream might be single (though this is unlikely), double, triple, four-fold, or even five-fold, with a corresponding number of drops.

Observations were next made on the effects of chords. For the chord of the fifth the pitches taken were 256 and $\frac{2}{3}\times 256$. The two forks could be screwed to the table and bowed, or, as is preferable (especially in the case of the chords of the fourth and third to be spoken of presently), maintained in vibration electromagnetically by a periodic current from a break-fork of pitch $85\frac{1}{3}$, standing on another table. The revolving disk was driven at such a speed as to give about eighty-five views per second. As was to be expected, the number of drops was either 256 in a triple stream, or $\frac{2}{3}\times 256$ in a double stream, according to the relative intensities of the two vibrations. With the maintained forks the phenomenon is perfectly under control, and there is no difficulty in observing the transition from the one state of things to the other.

In like manner with forks 256 and $\frac{3}{4}\times 256$, driven by fork 64, and with sixty-four views per second, the stream is either triple or quadruple; and with forks 256 and $\frac{4}{5}\times 256$, we get at pleasure a four-fold or five-fold stream. To obtain a good result the intervals must be pretty accurately tuned. In the case of electrically maintained forks, the relative phase remains unchanged for any length of time, and the spectacle seen through the revolving holes is one of great beauty.

The actual results obtained experimentally by Plateau differ in some respects from mine, doubtless in virtue of the more composite character of the notes of the violoncello employed by him, but they are quite consistent with the views above expressed. The only point as to which I feel any difficulty relates to the single stream, which occasionally resulted from the action of the twelfth below the principal note. It seems improbable that this could have been a single stream of the kind that I obtained with some difficulty from a pure tone; indeed the latter would have been pronounced to be a double stream by an observer unprovided with an apparatus for intermittent views. I should rather suppose that the number of drops really corresponded to an overtone, and that from some accidental cause the divergence of what would generally be separate streams failed to be sensible.

The Length of the Continuous Part.

When a jet falls vertically downwards, the circumstances upon which its stability or instability depend are continually changing, more especially when the initial velocity is very small. The kind of disturbance to which the jet is most sensitive as it leaves the nozzle is one which impresses upon it undulations of length equal to about four and a-half times the initial diameter. But as the jet falls its velocity increases (and consequently the undulations are lengthened), and its diameter diminishes, so that the degree of instability soon becomes small. On the other hand, the kind of disturbance which will be effective in a later stage is altogether ineffective in the earlier stages. The change of conditions during fall has thus a protective influence, and the continuous part tends to become longer than would be the case were the velocity constant, the initial disturbances being unaltered.

I have made many attempts to determine the origin of the disturbances which remain in operation when the jet is protected from ordinary tremors, but with little result. By suspending the reservoir with india-rubber straps, &c., from the top of a wooden tripod, itself resting upon the stone floor of one of the lower rooms of the Cavendish Laboratory, a considerable degree of isolation was attained. A stamp of the foot upon the floor, or the sounding of a note of suitable pitch of moderate intensity in the air, had no great effect. Without feeling much confidence I rather incline to the opinion that the residual disturbances are of internal origin. As the fluid flows up to the aperture along the inner surface of the plate which forms the bottom of the reservoir, eddying motions are almost certainly impressed upon it, and these may very possibly be the origin of the ultimate disintegration. With the view of testing this point, I arranged an experiment in which the velocity of the fluid over the solid walls should be as small as possible.

AB (fig. 1) represents a large brass tube, to which a smaller one is soldered at B, suitable for india-rubber connexion. The bottom of

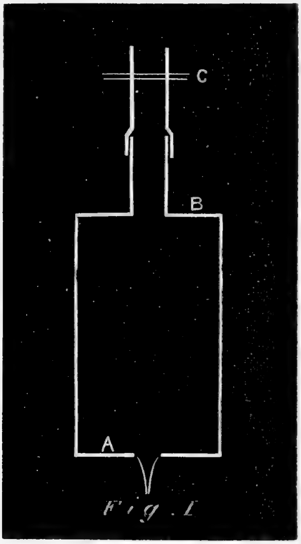


Fig. 1

the large tube consists of a carefully worked plate in which is a circular hole of $\frac{1}{2}$ inch diameter. When the rubber tube is placed in connexion with the water supply, a jet drops from A, and may be made exceedingly fine by regulaton of the pinch-cock C. By turning off the supply at C altogether, the jet at A may be stopped, without emptying the vessel. The stability, due to the capillary tension of the surface at A, preponderates over the instability due to gravity. By this device it is possible to obtain a jet whose velocity is acquired almost wholly *after* leaving the vessel from which it issues. In this form of the experiment, however, the jet is liable to disturbance depending upon the original velocity of the fluid as it passes through the comparatively narrow rubber tube, and when I attempted a remedy by suspending a closed reservoir (fig. 2), in which the water might be allowed first to come to rest, other difficulties presented themselves. The air confined over the surface of the water acts as a spring, and the flow of water below tends to become intermittent, when rendered sufficiently slow by limiting the admission of air. A definite cycle is often established, air flowing in and water flowing out alternatively at the lower aperture. The difficulty may be overcome by careful manipulation, but there is no easy means of making an adequate comparison with other jets, so that the question remains undecided whether the residual disturbances are principally of internal or of external origin.

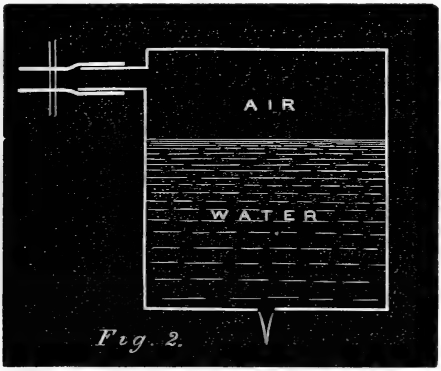


Fig. 2.

Collision of two Resolved Streams.

§4. In the case of a simple vertical fountain, when the scattering is prevented by electricity, there is every reason to believe that the action is differential, depending on a difference of potentials of colliding drops. The principal electrification, however, of the successive drops must be the same; and thus, sensitive as it is, this form of the phenomenon is not by any means the best calculated to render evident the smallest electrical forces. As was shown in my former paper, it is far surpassed by colliding *jets*, between which a difference of potential may be established, a subject to which we shall return in § 5. It is possible, however, to experiment upon the collision of two distinct streams of drops, which are differently,—if we please, oppositely—electrified from the first. Apart from electrical influence, the collision of such streams presents points of interest which have been made subject of examination.

Two similar brass nozzles, terminating in apertures about $\frac{3}{50}$ inch in diameter, were supplied from the same reservoir of water, and were held so that the jets rising obliquely from them were in the same plane and crossed each other at a moderate angle. The jets were resolved into regular series of drops by the action of a 256 fork screwed to the table and set in action by bowing. The periodic phenomenon thus established could be examined with facility by intermittent vision through a revolving perforated disk (§ 2), so arranged that about 256 holes passed the eye per second.

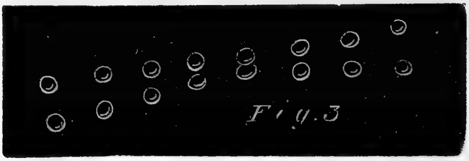


Fig. 3

When the angle of collision is small, the disposition of the files of drops may be made such that they rebound without crossing, fig. 3; more often, however, the drops shoulder their way through after one or more collisions, somewhat as in fig. 4. In both cases the presentation of an electrified body to one place of resolution will determine the amalgamation of colliding drops, with of course complete alteration of the subsequent behaviour. By judicious management a feebly electrified body may be held in an intermediate position

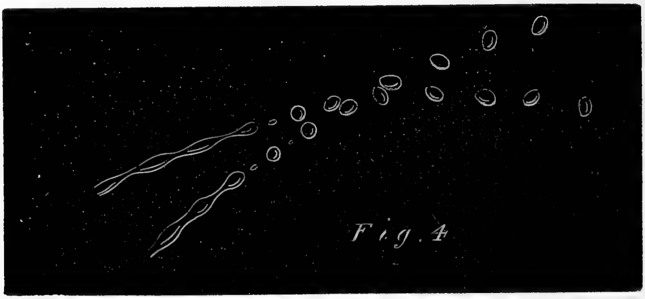


Fig. 4

between the two points of resolution so as not to produce the effect, confirming the view that the action is differential.

At a somewhat higher angle of collision amalgamation will usually occur without the aid of electricity, but the fact may easily escape recognition when intermittent vision is not employed. The streams do not usually join into one, as we might perhaps expect, but appear to pass through one another, much as if no union of drops had occurred. With the aid of the revolving disk the course of things is rendered evident. The separating layer is indeed ruptured at contact, and for a short time the drops move as one mass. There is, however, in general, considerable outstanding relative velocity, which is sufficient to bring about an ultimate separation, preceded by the formation of a ligament (fig. 5). In certain cases, although after contact a ligament is formed, the relative velocity is insufficient to overcome its tension, and the drops draw again together and ultimately cohere. If the impact is very direct, so that the relative velocity is almost entirely in the line of centres, the drops may flatten against one another and become united without the formation of a ligament.

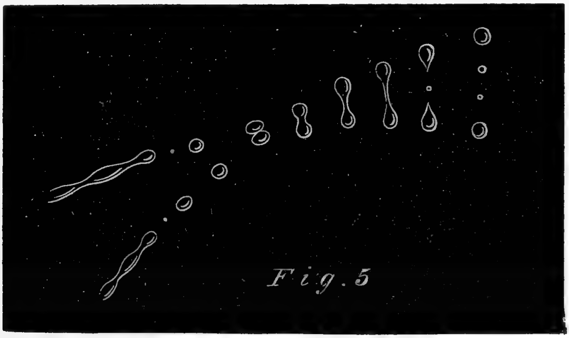
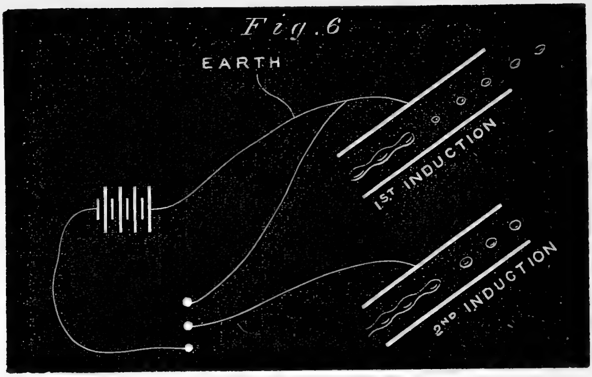


Fig. 5

In order to determine how small a difference of potential would be effective in causing the coalescence of streams of drops meeting at a small angle, the two places of resolution were enclosed in inductor-tubes, between which with the aid of a battery a difference of potential could be established. The arrangement is shown in fig. 6. One of the inductors is placed in connexion with the earth, with the reservoir from which the water comes, and with one pole of the battery. By operating a key, the other inductor may be placed at pleasure in communication with the first inductor, or with the other pole of the battery. In the first case the battery is out of use, and in the second the difference of potential due to the battery is established between the two inductors.

Fig. 6



Experiment showed that the effect depends a good deal upon the exact manner of collision. In almost all cases twenty cells of a De la Rue battery sufficed to produce amalgamation, with subsequent replacement of the original streams by a single one in a direction bisecting the angle between the original directions. With a less battery power the result may be irregular, some of the drops coalescing and others rebounding. When the collisions are very direct, even four cells will sometimes cause a marked transformation.

The complete solution of the problem of the direct collision of equal spheres of liquid, though probably within the powers of existing mathematical analysis, is not necessary for our purpose; but it may give precision to our ideas to consider for a moment the case of a row of equal spheres, or cylinders, with centres disposed upon a straight line, and so squeezed together that the distances between the centres must be less than the original diameters. By the symmetry, the common surfaces are planes, and the force between contiguous masses is found by multiplying the area of the common surface by the internal capillary pressure. When the amount of squeezing is small, the internal capillary pressure is approximately unaltered, and the force developed is simply proportional to the area of contact. In the case of the cylinder the problem admits of very simple solution, even when the squeezing is not small; for, as is easily seen, the free surfaces are necessarily semicircular, and thus the condition of unaltered volume is readily expressed. It will of course be noticed that as regards lateral displacements the equilibrium is unstable.

Collision of Streams before Resolution.

§ 5. The collision of unresolved streams was considered in my former paper. It appeared that the electromotive force of a single Grove cell, acting across the common surface, was sufficient to determine coalescence, and that the addition of a small quantity of soap made rebound impossible. Moreover, the "coalescence of the jets would sometimes occur in a capricious manner, without the action of electricity or other apparent cause."

As in many respects this form of the phenomenon is the most instructive, I was desirous of finding out the explanation of the apparent caprice, and many experiments have been made with this object in view. The observations on fountains recorded in § 1 having suggested the idea that the accidental presence of greasy matter, removable by caustic potash, might operate, this point was examined.

"*July* 8, 1880.*—*Colliding Jets.*—Two large glass bottles, with holes in the sides, close to the bottom, were fitted by means of corks with glass tubes, drawn out to nozzles of about $\frac{3}{40}$ of an inch in diameter. The bottles were well rinsed with caustic potash, to remove any possible traces of grease, and filled with tap water. The colliding jets coalesced in a manner apparently entirely capricious, the only principle observable being that they coalesced even more readily with high pressures (12 inches) than with low, and with lower pressures would stand collision at greater angles. The addition of caustic potash sufficient to give a very decided taste to the water, produced no apparent effect." Subsequently the water used was boiled with caustic potash, but without success.

"July 27, 28, 29, 30.—On the theory that when the jets collide without uniting there is between them a thin film of air, which would be very liable to be sucked up by water not saturated with air, we tried jets of water through which a stream of atmospheric air had been passed for several hours. We tried it three times. The first time the jets seemed very decidedly less liable to unite capriciously. The second time they behaved even worse than ordinary tap water usually does. The third time we thought it rather better than tap water usually is, but not materially so."

Jets of hot water, and of mixtures of alcohol and water in various proportions, were also tried at this time, but without obtaining any clue as to the origin of the difficulty.

I had begun almost to despair of success, when a determined attempt to conjecture in what possible ways one part of the stirred liquid could differ from another part suggested the idea that the anomalies were due to dust.

"Aug. 1880.—We tried dropping dust on to the colliding jets just above the point of collision, and found that union was always produced. The following powders were tried—powdered cork, sand, lycopodium, plaster of Paris, flowers of sulphur, sugar, dust that had accumulated upon a shelf, and later emery and putty powder. The lycopodium was a little more uncertain in its action than the others, but apparently only because, owing to its lightness, it was difficult to ensure its falling upon the jets. Whenever we were sure it did so, union followed."

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* Mrs. Sidgwick's "Note Book."

When mixed with the water, powders acted differently. Emery and putty powder were not effective, but sulphur caused immediate union. Much probably depends upon the extent to which the extraneous matter is wetted. A precipitate of chloride of silver, formed in the liquid itself, seemed to be without influence.

Acting upon this hint, Mrs. Sidgwick made an extended series of observations upon the behaviour of jets composed of water which had been allowed to settle thoroughly, and which were protected from atmospheric dust. For this purpose the jets were enclosed in a beaker glass, the end of which was stopped by a plug of boxwood, fitted air-tight. Through the plug passed horizontally the two inclined glass nozzles, and underneath a bent tube serving as a drain. The results, observed under these circumstances, were such as to render it almost certain that dust is the sole cause of the capricious unions. The protected jets of settled water were observed for a total period of 246 minutes, during which the unions were at the average rate of one in ten minutes. The longest intervals without unions were thirty-four minutes and twenty-nine minutes. Comparative experiments were made upon the behaviour of jets from the same nozzles under other conditions. Thus jets of unsettled water, but protected from atmospheric dust, united on an average twenty-four times in ten minutes. With unsettled water the protection from atmospheric dust is not of much use, as unprotected jets of the same water did not unite more than twenty-six times in ten minutes. On the other hand, jets of settled water, not protected from the atmosphere, united only twelve times in ten minutes. Although, no doubt, somewhat different numbers might be obtained on repetition of these experiments, they show clearly that the dust in the water is the more frequent cause of union under ordinary circumstances, but that when this is removed the atmospheric dust still exerts a powerful influence. The difficulty of getting water free from dust is well known from Tyndall's experiments, so that the residual tendency to unite under the most favourable conditions will not occasion surprise.

Although there is no reason to suppose that any other cause than dust was operative in the above experiments, it remains true that very little impurity of a greasy character will cause immediate union of colliding jets. For this purpose the addition of milk at the rate of one drop of milk to a pint of water is sufficient. It may be noticed too that the effect of milk is not readily neutralised by caustic potash.

With respect to the action of electricity, further experiments have been made to determine the minimum electromotive force competent to cause union. The current from a Daniell cell was led through a straight length of fine wire. One end of the wire was connected by platinum foil with the liquid in an insulated glass bottle, from which one of the jets was fed. The glass bottle supplying the second nozzle was similarly connected with a moveable point on the stretched wire. The electromotive force necessary to cause union, as measured by the distance between the two fine wire contacts, though definite at any one moment, was found to vary on different occasions, possibly in consequence of forces having their seat at the surfaces of the platinum oil. From one-half to three-quarters of the whole force of the Daniell was usually required.

With a view to further speculation upon this subject, an important question suggests itself as to whether or not there is electrical contact between colliding and rebounding jets. To solve this question it was only necessary to introduce a fine wire reflecting galvanometer into the arrangement just described, taking care that the electromotive forces employed fell short of what would be required to cause the union of the jets. Suitable keys were introduced for more convenient manipulation, and sulphuric acid was added to the water, in order to make sure that absence of strong galvanometer deflection could not be due merely to the high resistance of the thin columns of water composing the jets. Repeated trials under these conditions proved that so long as the jets rebounded their electrical insulation from one another was practically perfect.

As to the explanation of the action of electricity in promoting union, it would be possible to ascribe it to the additional pressure called into play by electrical attraction of the opposed water-surfaces, acting as plates of a condenser. But it appears much more natural to regard it as due rather to actual disruptive discharge, by which the separating skin is perforated, and the equilibrium of the capillary forces is upset. A small electromotive force, incapable of overcoming the insulation of the thin separating layer, is without effect.

XIV. "On a Collection of Rock Specimens from Socotra." By Professor T. G. BONNEY, M.A., F.R.S., F.G.S. Received June 12, 1882.

(Abstract.)

In the spring of 1879 the island of Socotra, which lies off the north-east corner of Africa, about 140 miles from Cape Gardafui, was visited by Professor Bayley Balfour. Landing at the north-west extremity, he traversed the northern side of the island up to the eastern end, then returning by a more central course to the sea, he

crossed the Haggier Mountains to the southern coast, and returned again to Hadibu, on the north side, by a route lying further to the west. During this journey, in addition to extensive botanical and zoological collections, Professor Balfour obtained about 500 rock specimens illustrative of the geology of a considerable portion of the island. These were sent to the author for examination. A considerable number of them, as was to be expected, were more or less weathered, and so were not in a very favourable condition for precise description; but about eighty of the best preserved specimens have been examined microscopically; from the study of which, and of the remainder the following sketch of the geology of the island may be given.

The north-west, inland from Gollonsir Bay, consists of a plateau of limestone resting unconformably upon a group of highly crystalline gneisses, associated with diorites, which correspond in general character with the Hebridean series of north-west Scotland. The latter group is frequently exposed in the beds of the valleys, the uplands on either side being formed of the limestone. The elevated district traversed between Gollonsir and Kuhmeh Bays is similarly constituted, but it is probable that some true granite also exists among the older series; the limestone extends all along the coast of the latter bay, having its usual foundation, and there is evidence that felsites occur somewhere in this district, most probably inland from the eastern shore. In this part are basalt dykes, which cut the limestone as well as the older rocks.

Near the coast of Hadibu Bay, west of that town, we have limestone, conglomeratic at base, resting on an indurated shale or argillite, together probably with an intrusive rock approaching kersantite in character. The argillite is also found inland beneath the limestone, south-east of Hadibu. The Haggier mountains, a fine chain forming a sort of backbone to the island, consist of felspathic granites, varying from coarse to fine, the former containing little besides quartz and felspar (the variety pegmatite), through which have broken minette, basalt, and felsite; the limestone may be traced some distance up their flanks. East of the Haggier, the granite rock continues, but quartz-felsites, and even rhyolites, appear to become more common, and an epidotic quartzite gives an indication of the occurrence of the metamorphic group. Granite and felsites form the inland district traversed by the river which passes Maaber, as well as the eastern half of the Haggier mountains.

The district in the neighbourhood of the coast between this and the next river to the east, consists of granite cut by felsites, rhyolites, and diorites, or dolerites. Possibly the gneissic series reappears here. Further east yet we obtain clearer indications of the latter, overlain as before by an extensive capping of limestone. Thus, the main axis of the northern part (if not of the whole island), appears to consist of granitoid gneiss, replaced towards the centre by granite.

The granitic, felsitic, and rhyolitic rocks must occupy a considerable breadth of the island from north to south, for there are many specimens from districts traversed on the return journey from the eastern promontory, in which Prof. Balfour, after keeping parallel to the southern coast for some miles, struck inland in a north-west direction. Thus measured, there must be an area some ten miles across occupied by these rocks; and judging from the specimens, one would say that this was one of the chief centres of ejection of rhyolitic lavas; this is near that part of the island covered by the final A in SOKOTRA on the map. In crossing back to the north shore along the course of the Haggier river granites, basalts, felsites, and rhyolites, as might be expected, were collected. The conglomerates of felsite and rhyolic pebbles picked up on the Nowkad Plain, approaching the southern coast, show that there must be a large mass of these rocks somewhere on the south flank of the Haggier range.

The limestone is generally of a yellowish or whitish colour, compact in structure, and often not unlike the dolomites of the Italian Tyrol, in the hand specimen. Microscopic examination shows that it is sometimes partially dolomitized. It contains numerous foraminifera amphistegina, globigerina, textularia, rotalina, &c. The first of these shews that it is probably of Middle Tertiary age, and thus rather later than the limestone of the Sinai Peninsula.

The author's investigations lead then to the following conclusions: That the oldest rocks in Socotra are gneisses, hornblendic, and granitoid, belonging, like those of the north-west of Scotland, of North-east America, &c., to the earliest Archæan age. That these, as at Sinai, are broken through by granites, some of which resemble much those of Serbal and Jebel Musa, and that these are cut by later granites, felsites, and greenstones, together with basalts, the last probably of rather recent date. On the southern flank of the Haggier range, there must have been a rather extensive volcanic disturbance, from which rhyolitic lavas, often showing marked fluidal structure and scoria were ejected. The date of these eruptions cannot be fixed, but it was prior to the deposition of the limestone, and may be much older, except locally, where there is a little sandstone possibly representing the Nubian sandstone (Carboniferous) of Sinai, and the argillite. What is now Socotra, would appear to have been a land surface from very early times, until the submergence in the Miocene period, when the great masses of limestone were deposited. It is, however, quite possible that the peaks of the Haggier range may have remained above water even during that time. Since its elevation, great denudation has doubtless taken place, including the definition of the island, and the sculpturing of the valleys in the limestone district. During this period, there have been some disturbances of a volcanic nature, as the limestone is cut by dykes of basalt, and of compact trachytes which, however, differ considerably from the purplish rhyolites already mentioned. As there is a possibility of this island having remained above water from a very remote antiquity, the investigation of its flora and fauna will possess a peculiar interest.

XV. "On the Photographic Spectrum of Comet (Wells) I, 1882." By WILLIAM HUGGINS, D.C.L., LL.D., F.R.S. Received June 15, 1882.

On the evening of Wednesday, May 31, I obtained a photograph of the spectrum of this comet with an exposure of one hour and a quarter. A spectrum of α Ursæ Majoris was taken through the other half of the slit, on the plate, for comparison.

The photograph shows a strong continuous spectrum extending from about F to a little beyond H. In this continuous spectrum I am not able to distinguish the Fraunhofer lines. In this comet therefore, at this time, the original light giving a continuous spectrum must have been much stronger relatively to the sunlight reflected than was the case in the comet of last year. It should be stated that the greater faintness of the present comet made it necessary to use a more open slit, which would cause the Fraunhofer lines to be less distinct; but the lines G, H, and K are to be clearly seen in the star's spectrum taken under the same conditions.

Eye observations by several observers on the visible spectrum of the comet had already shown that this comet for the first time since spectrum analysis was applied to the light of these bodies in 1864, gives a spectrum which differs essentially from the hydrocarbon type to which all the comets previously examined spectroscopically (about twenty) belong.

In the visible spectrum bright lines, presumably of the vapour of sodium, and some other bright lines and bright groups of lines have been seen. The hydrocarbon bands in this part of the spectrum have been suspected to be present by some observers.

The photographic spectrum differs greatly from that of the comet of last year.* I am not able to see the cyanogen group in the ultra-violet beginning at wave-length 3883, nor are the other two groups between G and *h* and between *h* and H to be detected.

The continuous spectrum which extends from below F to a little distance beyond H, contains at least five brighter spaces, which are doubtless groups of bright lines, though it is not possible in the photograph to resolve them into lines. These places of greater brightness can be traced beyond the border of the continuous spectrum on the side which corresponds to the coma of the comet on the side next the sun. The light from this part of the comet gave a very much fainter continuous spectrum, for on the photographic plate it appears to be almost wholly resolved by the prism into these bright groups. One or two fainter groups are suspected to be present, but they are too indistinct to admit of measurement.

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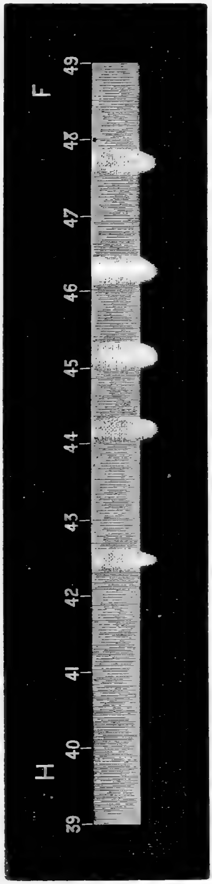
* "Proc. Roy. Soc.," vol. 33, p. 1.



The five stronger bright groups are too faint at the commencement and ending of each group to permit of more than a measurement of the estimated brightest part of each bright space.

The positions of these brightest parts are—

λ 4769,
λ 4634,
λ 4507,
λ 4412,
λ 4253.

Professor A. Herschel and Dr. von Konkoly pointed out long ago that the spectra of periodic meteors belonging to different swarms differ from each other, and the meteorites which come down to us differ greatly in their chemical constitution. It is not surprising to find the matter of the nucleus of this comet to exhibit a chemical difference from that of other comets.

In the diagram, the width of the continuous spectrum corresponds to the diameter of the nucleus. The bright bands extend into the coma on the side next the sun.

XVI. "On the Action of Heat upon the Contagium in the two forms of Septichæmia known respectively as 'Davaine's' and 'Pasteur's.'" By G. F. Dowdeswell, M.A. (Cantab.), F.L.S., F.C.S., &c. Communicated by J. Burdon Sanderson, M.D., LL.D., F.R.S., &c. Received June 15, 1882.

Professor Rosenberger, of Würzburg, has recently published the results of experiments,* by which he claims to have effectually sterilised by heat, the blood and exudation fluids of the rabbit in the two forms of septichæmia, known as those of Davaine and Pasteur; and he states that these fluids so sterilised, upon injection into other animals, were found to be infective, reproducing the disease with the recurrence of the specific organisms which characterise it: he therefore regards these organisms as having no causal connexion with the affections in which they are found, but as merely secondary or epiphenomenal. That this would be the necessary deduction from the experiments mentioned, if it were proved that the fluids had been effectively sterilised, is obvious; but the account published contains no details whatever of the methods employed, nor protocol of the experiments, so that it is impossible either to discuss them or to form a judgment as to the correctness of the conclusions. They, however, involve a question so important in respect to the theory of contagium vivum—the relations of these micro-organisms to disease—that it was determined to work out the subject on the basis indicated in Professor Rosenberger's paper, adopting such methods and precautions as appeared necessary.

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* "Centralb. f. d. Med. Wiss.," 1882, No. 4, pp. 65-69.

PART I.—*Experiments upon Pasteur's Septichæmia in the Guinea-Pig.*

Guinea-pig No. 1.—0·7 cub. centim. of putrid ox-blood was injected with a Pravart's syringe, into the peritoneal cavity of a full-grown guinea-pig, which the next morning was found recently dead, rigor not having set in: round the place of injection there was some subcutaneous exudation, with destructive inflammation of the tissues of the abdominal wall, sections of which showed numerous Bacilli and Micrococci in the layers of connective tissue between the muscles. Acute peritonitis was found with a large exudation of serous fluid containing some extravasated blood-corpuscles, and deeply stained with their colouring matter. The fluid in this case was not very coagulable, differing in this respect from some others. The same day 0·5 cub. centim. of the peritoneal exudation fluid of No. 1 was injected into the subcutaneous tissue of the abdomen of guinea-pig No. 2, which, as the following day was Sunday, was not examined till Monday morning, when it was found dead, and in a much more advanced stage of decomposition than would have occurred normally in the same period. In all forms of septichæmia this rapid decomposition is invariably found. Guinea-pig No. 3 then received in similar manner 0·5 cub. centim. of the diluted subcutaneous exudation fluid of No. 2, which, likewise, was not coagulated. On the following morning No. 3 was found dead but still warm; the abdomen was infiltrated with a large quantity of subcutaneous exudation fluid, deeply stained with hæmoglobin, and containing some extravasated blood-corpuscles, as well as numerous active Bacilli and some Micrococci or spores. The serous fluid was not very coagulable; of this a portion was mixed with equal parts of normal saline solution, freshly made and boiled, containing in addition 1 per cent. of potassic carbonate, to render the serum alkaline and prevent coagulation upon boiling, which it did effectually. Vacuum tubes of about 10 millims. diameter and 10 centims. long, previously prepared, were then partially filled with this liquid by breaking their points under its surface; these were then placed one by one in a small flask of salt solution, to avoid frothing and splashing; this was then heated, and when the liquid was boiling freely, the tube enclosing it was resealed by the blow-pipe. The tubes were then placed in the hot air chamber, the temperature of which was gradually raised to 140° C., and maintained at nearly that point for one hour. No coagulation occurred upon heating, but it was found in subsequent experiments, that, inasmuch as the degree of dilution and alkalinity required to prevent this, varied in different cases, both with exudation serum and with blood, it was necessary in every instance to determine this point experimentally. The same evening guinea-pig No. 4 received by subcutaneous injection 0·3 cub. centim. of the exudation fluid of

No. 3, diluted and prepared as above mentioned, but unheated, while guinea-pig No. 5 received in similar manner 0·3 cub. centim. of the same fluid superheated and cooled. The following day, twenty-five hours after injection, No. 4 was prostrate, in a state of collapse, and the next morning was found dead with the same symptoms and micro-organisms as in previous cases, while No. 5 was quite unaffected, feeding heartily and with temperature unaltered. This animal remained healthy and unaffected for some days, and subsequently died from an accidental cause. On examination no symptoms whatever were found, either of septichæmia or peritonitis; there was no exudation of any sort and the blood was free from organisms. The following day the exudation fluid of No. 4 was diluted and rendered alkaline in the same manner as in the previous experiments, enclosed in tubes and superheated as before; of this 1·0 cub. centim. was injected into the subcutaneous tissue of guinea-pig No. 6, which, notwithstanding the large quantity it received, remained totally unaffected and was ultimately killed.

Some cultivation experiments were then made in different nutrient fluids, both with the sterilised and the unsterilised exudation serum, the result was that in all cases excepting one, in which there was reason to believe that the result was due to accidental contamination, no development of organisms occurred with the sterilised fluid, which it invariably did with the unsterilised.

In the septichæmia of Pasteur, the characteristic organism is a Bacillus, somewhat similar morphologically to the *B. anthracis*, and one of the forms of the hay Bacillus, *B. subtilis* of Cohn, the ubiquitous organism which developes so readily and constantly in most organic infusions, from atmospheric or other contamination, and until the specific morphological characters of these organisms are better discriminated than they are at present, that is probably until there is a further substantial advance in our optical appliances, it is scarcely safe to draw any conclusion from their occurrence in cultivations, unless these are numerous and repeated, with rigorous control experiments.

In this case, *i.e.*, Pasteur's septichæmia, or the malignant œdema of Koch, in the guinea-pig, I have invariably found both in this, as in other series of experiments, that in animals examined immediately upon death there are none of the specific organisms (the Bacillus described,—which is so large that it can scarcely be overlooked) to be found in the blood, or in any of the organs of the infected animal, and that the blood is not infective. After death, however, they speedily invade the organs of the animal, the kidneys and the spleen being apparently the first attacked; hence the necessity in all these cases of examining the subject immediately after death.

PART II.—*Experiments upon the blood of Rabbits in the form of Septichæmia known as that of Davaine.*

This form of septichæmia in the rabbit, which has attracted so much attention, and been the subject of so many experiments since those first published by Davaine and by MM. Coze and Feltz, in 1869, is originated by the subcutaneous injection of a small quantity—a few drops—of putrid blood, usually that of the ox. Infection with the specific disease in this case, as in the parallel one of septichæmia in the mouse, is somewhat uncertain, and the law on which it depends has not been clearly determined. Usually, however, blood which is only a few days old,—three or four—is, as originally stated by Davaine, the most readily infective, but by no means constantly so. In the following experiments all the rabbits employed were young, and nearly of the same age and size, not quite full grown. The septichæmic blood used for injection was always diluted, generally with an equal bulk of freshly prepared and boiled normal saline solution; the quantities of blood given as injected, unless otherwise stated, are always those of the blood itself which was used, and not of the solution.

No. 1.—0·2 cub. centim. of putrid ox-blood some days old was injected into the subcutaneous tissue of the back of rabbit No. 1. In forty-eight hours afterwards the rectal temperature was found to be 106° F., and the animal died on the third day. In the blood from the heart, examined immediately after death, a few of the organisms characteristic of the disease* were found, and preparations made and stained by the Weigert Koch method, gave the same result.

Two hours after death 0·1 cub. centim. of the blood of No. 1 was injected into rabbit No. 2. Twenty hours afterwards the rectal temperature was 104° F., and the animal expired in my presence twenty-three hours after injection. Section was made almost immediately, within about five minutes after death, but the blood was found already partially coagulated in the cavities of the heart and in the vessels. The blood which remained fluid was quickly mixed with two parts normal saline solution, with the addition of 3 per cent. pot. carb., and 0·3 cub. centim. of this solution (=0·1 cub. centim. blood) was injected into the subcutaneous tissue of rabbit No. 3, which died just within twenty hours after injection. A portion of the dilute alkaline blood was then placed in tubes, by breaking their points, sealed at a red heat, underneath the fluid; they were then immersed in a small flask of saline solution, sealed while boiling, and placed in the hot air oven, the temperature of which was gradually raised to 140° C., and slowly cooled; it was maintained at a temperature of over 100° C. for upwards of one hour. It being then late in the evening, rabbit No. 4 was on the next morning injected with a Pravart's syringe full of the dilute superheated blood. This animal survived and was but slightly, if at all affected, notwithstanding the very large quantity of fluid injected. The following day there was no appreciable disturbance whatever, but on the third day slight pyrexia occurred, the animal's skin felt hot to the touch, and the rectal temperature rose to 100·0° F., which, however, sometimes occurs normally in rabbits kept in confinement. Subsequently, it remained totally unaffected for several days, as long as observed, and was then destroyed.

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* These are fully described in the forthcoming number (for June, 1882) of the "Journ. Royal Micros. Society."

Some days afterwards, the blood of another rabbit of a subsequent generation of the same infection, immediately upon death, was mixed with two parts normal saline solution, and 2 per cent. pot. carb., this was inclosed in tubes, boiled, sealed, and heated to 120° C., in the same manner as before.

Of the alkaline dilute blood (unheated) further diluted up to 100,000,000 times, 0·6 cub. centim. (=·000000006 cub. centim. of blood) was injected into the back of rabbit E, which died of septichæmia within twenty hours.

Of the dilute superheated blood, 0·6 cub. centim. (=0·2 cub. centim. blood) was injected into rabbit F, which the next day was apparently unaffected, but died with the usual symptoms of septichæmia the following day, *i.e.*, thirty-nine hours after injection. In the blood were found the characteristic Bacteria, and it proved to be infective when injected in a small quantity into another rabbit. Upon this, the same day, a further portion of the same superheated dilute alkaline blood was injected in the same quantity (0·6 cub. centim.) into rabbit G. This rabbit remained totally unaffected, and survived as long as observed, twenty days after, although it had received upwards of 30,000,000 times the quantity of blood which had proved lethal to rabbit E within twenty hours.

Cultivation experiments have been made both with the superheated blood and with that in the natural state, taken upon death; but as it was found that the Bacterium which occurs in these cases refuses to germinate in any of the various cultivating media employed, excepting only in the serum of ox-blood, and in that very sparsely and uncertainly, they were inconclusive; and the greater part of the cultivating glasses inoculated with blood of both sorts, after being kept several days in the incubator at temperatures between 30° and 40° C., unstable as the solutions are, remain to this day—a month subsequently—perfectly pellucid and unaltered, save from some loss by evaporation.

On considering these results, it appeared possible that, in the case where the superheated blood proved infective, from the method of filling the vacuum tubes, portions of the dilute blood drying on their sides, might have escaped perfect sterilisation by heat; it was therefore determined to repeat the experiments with tubes filled by a method which should avoid this possible source of error. Tubes were made of thick German glass, about ·3 inch internal diameter, closed at one end and slightly drawn out three or four inches from the bottom; then the blood of rabbit H, which died in my presence, of transmitted infection, was immediately collected and mixed with three times its bulk of normal saline solution with the addition of ·3 per cent. potassic carbonate. In this instance it was found that a less degree of either alkalinity or dilution would not prevent coagulation on heating. Of this blood further diluted up to the ten-thousandth degree, 0·6 cub. centim. (=·00006 cub. centim. of the unmixed blood) was injected into the back of rabbit I, which died of septichæmia in twenty-seven hours; the characteristic organisms were found in the blood, which on inoculation into another rabbit proved to be infective. A portion of the same alkaline dilute blood was then placed in the tubes above-mentioned, 2 to 3 cub. centims. in each, by means of a capillary pipette introduced to the bottom, contamination of the sides being avoided; the tubes were then drawn out by the blow-pipe to a capillary point, boiled in salt solution and sealed while boiling. One tube that was heated separately to 140° C. burst, as had been the case in several other experiments previously; the remainder were thereupon heated to fully 100° C. for six hours, when the temperature was raised to 130° C., were maintained at that for one hour, and then gradually cooled. Of this dilute superheated blood, rabbit K received by subcutaneous injection in the back 0·6 cub. centim. (=0·15 cub. centim. blood), and rabbit L received 1·1 cub. centim. of the same (=0·275 cub. centim. blood), that is nearly five thousand times as much as rabbit I had received of the unheated blood. Both these rabbits K and L were unaffected in any manner whatever: there was no appreciable variation of the rectal temperature; they continued to feed well, and did not lose flesh. They were killed ten days subsequently; and no material inflammation at or around the place of injection, and no thickening whatever of the tissues which occurs so markedly in all cases of infection could be observed; there was only a slight stain, as if by the colouring matter of the blood, in the subcutaneous tissue near the spot of injection.

From the result of these experiments I conclude that the active virus of infection, both in the case of Pasteur's septichæmia—the malignant œdema of Koch—in guinea-pigs, and in Davaine's septichæmia in rabbits, is destroyed by the prolonged action of a sufficiently high temperature; that blood or exudation fluid so treated is not infective, nor in any appreciable manner toxical, when injected in moderate quantities (up to 1 cub. centim.) into other healthy animals, while the same fluids unheated, are invariably and fatally infective in infinitely smaller quantities.

In Davaine's septichæmia in the rabbit, I have found throughout these experiments that the period of incubation is remarkably constant, death, after the first generation of infection by putrid blood, almost invariably occurring from about twenty to twenty-five hours, and consequently that if it does not occur within about that period, it may be concluded that infection has failed; it may die subsequently, as rabbits in confinement constantly do, more especially under the conditions in which they are kept in laboratories; but unless within about the period specified they do not die infected with specific septichæmia, the characteristic organisms are not found in the blood, nor is that blood infective; hence it is not necessary to observe such animals for more than a few days after inoculation.

These experiments were conducted in the laboratory at University College, with the co-operation of Dr. Burdon Sanderson, and at his suggestion.

XVII. "On the Development of the Enamel of the Teeth of Vertebrates." By EMILY NUNN. Communicated by Professor HUXLEY, F.R.S. Received June 14, 1882.

[PLATES 2–4.]

The question of the origin of the enamel of the teeth in vertebrate animals has been the subject of much discussion. It has been held—1st, that the enamel results from the calcification of the enamel cells (Tomes, Waldeyer, Edwards); 2nd, that it is formed by excretion from those cells (Kölliker, Hertwig, Leydig); and, 3rd, that it is not formed by these cells at all, but has the same origin as the dentine, whatever that may be.*

Again, there are numerous opinions concerning the nature and origin of the *cuticula dentis*:—1, that the cuticula is the persistent basement membrane itself (Huxley, Hertwig, Leydig); 2, that it is the altered enamel membrane; 3, that it is the external layer of cells of the enamel organ (Waldeyer); 4, that it is the metamorphosed *stratum intermedium* of the enamel organ (Löwve); 5, that it is the ends (*Deckeln*) of the cells of the enamel membrane (Köllman); 6, that it is a dermic structure (Tomes); 7, that it is an excretion of the cells of the enamel membrane (Kölliker).

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* Huxley, "Tegumentary Organs," "Encyclopædia of Anat. and Phys.," vol. v, Sup., gives reasons for considering the whole tooth ecderonic, but adds, "All these points can only be decided by a much more extensive series of investigations."

The existence of a basement membrane or *membrana præformativa*, said by some to cover the tooth papilla, is generally considered doubtful. It is asserted, 1st, that the newly-formed layer of enamel has been mistaken for it (Waldeyer); 2, that the first-formed layer of dentine has been mistaken for it (Löwve); 3, that the "appearances described are capable of a different interpretation" (Tomes).

The present investigation was undertaken at the suggestion of Professor Huxley with the hope of determining—or at least of getting more light upon—firstly, the history of the various membranes—the cuticula dentis, the so-called newly-formed layer of enamel, the membrana præformativa, and the first-formed layer of dentine; secondly, the origin of the enamel.

The present paper gives the results which have been arrived at concerning these points.

The nature of dentine will form the subject of a second paper.

Since it is well established that the teeth and placoids of the Plagiostomi are homologous with the teeth of the higher vertebrates, illustrations from these will be used as well as from mammalian teeth, adult and embryonic.

The drawings described have been made by the aid of the camera.

1. *The Cuticula Dentis.*—A membrane covering the enamel of young teeth has been repeatedly described as homogeneous and reticulated, or with "*zellzeichnungen*," supposed to have been made by adjoining cells. Huxley[*] says of this membrane: "It is perfectly clear and transparent, and under a high power exhibits innumerable little ridges upon its outer surface, which bound spaces sometimes oval, sometimes quadrangular, and about $\frac{1}{1500}$ of an inch in diameter. In the frog its surface is in parts reticulated as in man; in the mackerel and skate I have been unable to find any such reticulation. In the calf a similar membrane may be demonstrated, but it is much more delicate, and I have not seen the peculiar areolæ on its surface."

This account agrees with the description given by other writers of a membrane found lying upon the enamel, as also with my own observations.

The membrane, then, may be reticulated, entirely, in parts only, or not at all. It varies in thickness in the same animal, and as well as in different animals, and may be thick or delicate. It has been found by myself, as well as by other observers, upon the young tooth, both before and after its eruption. Hitherto it has not been figured,[†] neither has its history been determined; hence the various opinions as to its nature and origin. Waldeyer[‡] declared it to be the newly-formed layer of enamel. Huxley* considered it as the persistent basement membrane.

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[*] Huxley, "Quarterly Journal of Microscopical Science," 1853, pp. 157, 158.

[†] A drawing, by Nasmyth, has since been found, of the inner surface of this membrane, showing the impressions of the enamel prisms.

[‡] Waldeyer, "Handbuch," 1869.

Fig. 1, A, Pl. 2, is a drawing *en face* of this membrane as seen upon the molar of a young rabbit; it was taken from the tooth below the gum. The areolæ, upon its outer surface, were perfectly clear and transparent, as has been described, and a side view (fig. 1, B) showed them to be elevated into ridges. But the spaces or disks which they bounded were granular and were found to stain slightly with carmine. In this preparation they were round, but in some they are distinctly polygonal, while in others they are faintly seen and their outlines cannot be definitely traced. In the part of this membrane exposed above the gum, which appeared perfectly homogeneous, they could not be detected at all. While this membrane still covers the embryonic tooth "it may be that a few of the elongated cells of the organon adamantinæ adhere to it."† This may be the case even when the membrane has been well washed with a camel-hair brush previously to being hardened, as in fig. 1, B. This preparation clearly shows the reticulation to correspond in position to the cell-wall of the overlying cells and the enclosed spaces to the protoplasmic contents. The different structure of the clear colourless glassy areolæ and the granular slightly coloured disks, proves the reticulation to be something more than the impression of adjoining cells, as it has been described to be. The frequently granular character of the disks is rarely so distinct as in the present preparation, generally requiring a high power of the microscope for its demonstration, and it seems previously to have escaped observation.

The ridges, always upon the outer surface of the membrane, appear to be the walls of the cells, broken off a little above the extreme end of the cell, and in many preparations they occupy a larger relative space in the membrane than is shown in fig. 1.

It would seem as if the cell-wall were thickening about this end of the cell, or the protoplasm were undergoing a differentiation into a substance similar to cell-wall. The ends of the cells of the overlying enamel membrane frequently appear jagged (fig. 2), as also the surface of the membrane directed toward them, while its other surface, or that directed towards the enamel, is perfectly smooth, as is the case with the surface of the enamel, when the tooth has been sufficiently carefully and gradually decalcified.

In the upper part of fig. 2, where the tissues have not been separated, no membrane can be detected, the line between enamel cells and membrane not being a sharp one; but when the former are torn away, the latter is isolated (fig. 2, *c*), and a front view showed it to have the appearance of fig. 1, save that the disks are polygonal (fig. 2, B).

A similar structure on the tooth of the thornback is shown in fig. 3.

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* Huxley, *l.c.*

† Huxley, *l.c.* p. 157.

Here, as is frequently the case, the ends of the cells appear broken or vacuolated, or striated, and with high power the striæ can be distinctly seen in transverse section on the edge of the membrane, which in all stages, being much more tenacious than the enamel cells, generally adheres to the enamel when the cells are removed, and may be raised up by the use of acid. As a rule, the older the tooth the more completely the reticulations or the cell-markings are obliterated, and the more resisting is the membrane, so that by maceration, as well as by the help of an acid, it may be taken off entire from the surface of the young tooth. It has been isolated by myself in this way, as also by Hertwig, who asserts it to be the "*derb*" basement membrane, and the "*zellzeichnungen*" upon it to be simply the impressions of adjoining cells. Preparations similar to fig. 3 clearly disprove this, and the basement membrane, when it can be demonstrated, is far from a "*derb*" structure, as will be shown further on.

Kölliker* says—"The ends of the enamel cells taken from the enamel present different appearances. Some are simply cut off squarely, others possess smaller (myself, Hertz) or larger (Waldeyer) clear layers of the same breadth as the cells (the enamel fibres in process of formation), still others, finally, have pointed ends, with or without such layers (Tomes, Waldeyer, Hertz). I consider these ends, which I also have seen, as artificial products—that is, as accidentally detached parts of the yet unfinished enamel fibres." If Kölliker had seen them on the firm dense membrane, as in fig. 3, or fig. 2, he could not have held this opinion. An extended and careful study leaves no room for doubt that this membrane, forming in many cases the cuticula of placoids and the cuticula dentis of both mammalia and fishes, is made by the metamorphosed ends of the enamel cells. But the cuticula has not always precisely this structure either upon the tooth or on the placoid; apparently more or less of the so-called enamel cells may enter into its formation. This, at least, is certain, the cuticla, or "Schmelz oberhäutchen," may not only have upon its surface the "cell markings," but it may be formed of entire cells (or at least of enough to include the nuclei) which have undergone a greater or less differentiation into horny tissue, obscuring more or less completely the outlines of the nuclei. Such a cuticula is shown *in situ* on the placoid represented in fig. 4. It is entire upon the under surface of the scute, which is more protected; upon the upper side fragments only are left. A high immersion lens showed distinctly on parts of the membrane, the outlines of cells and their nuclei (fig. 5) and the membrane could be traced into the columnar layer of epithelial cells at the base of the spine.

The cuticula of the mammalian tooth has several times been found to have the same structure, and it has been possible, in transverse and longitudinal sections, to trace the gradual transition of the enamel cells into a perfectly homogeneous membrane (figs. 6 and 7, Plate 3), the cylindrical cells growing shorter as they approach the crown of the tooth, until, instead of being columnar, they are almost square, and finally flattened, and at last the outlines of the cells quite disappear, and there is left a perfectly homogeneous membrane.

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* Kolliker, "Handbuch der Gewebelehre," p. 385.

These changes are not easy to follow; in many preparations it is impossible to make anything out, and the drawings have been made from most fortunate preparations selected from some thousands of sections prepared in various ways.

The cuticula dentis, then, is formed by the metamorphosis of more or less of the enamel cells, and this metamorphosis may begin before any calcification of the underlying dental tissues. In this stage it has been frequently taken for the "newly-formed layer of enamel," for the "basement membrane," and for the "first-formed layer of dentine."

2. *The Basement Membrane and Membrana Præformativa.*

The mucous membrane of the Plagiostomes immediately under the epithelium is frequently more or less laminated, and one of these laminæ bounding the surface of the mucosa has often been described as the "*derb*" basement membrane. But being only the outer one of a series of laminæ, it represents the basement membrane as simply the margin of the mucosa, with no definite and special structure of its own.

In order to get at the special structure of the surface of the derma supposed to be bounded by a basement membrane, it is well first to examine portions which have a cellular rather than a distinctly laminated structure, and it is absolutely necessary that the sections should be perfectly vertical to the surface of the derma, the study of which will be much facilitated if the epithelial cells have, for the most part, been previously washed away.

Fig. 8 is a vertical section of the dermis and of the base of a young tooth of the skate. The section being very thin and perfectly vertical, the structure of the cellular granular dermis stands out in strong contrast to the perfectly clear and dense basement membrane, brought into even greater relief by the cleft (*cl.*), and by its absence from a part of the derma where it has been accidentally torn away. The basement membrane runs up from the derma over the surface of the young tooth, which, as yet, has no calcific deposit, though there is a thick *dentinal basis* under the basement membrane, much thicker than the future enamel. It is finely granular throughout, showing no differences of structure in different parts, no signs of tubules, though there is generally a faint granular striation running from the surface of the pulp to the basement membrane.

Fig. 9 is a portion of a vertical longitudinal section of the young tooth of a thornback hardened in alcohol. The basement membrane could be traced from the derma quite up to the apex on one side; it was quite invisible upon the other side, which was cut slightly obliquely to the surface of the tooth. The series of sections, however, furnishing the requisite conditions showed it to be there also. But in this section (fig. 9) not the slightest trace of it can be detected, and, but for subsequent sections, one could declare it did not exist.

If, at any time before calcific deposit, a young tooth* be carefully cut out, with a portion of the surrounding derma, and the cells of the enamel organ be washed away, examination with the microscope will show its outer surface to be quite smooth and dense,† and a clear strong homogeneous membrane, enveloping granular and cellular contents, can be pretty clearly made out. Pressure of the coverslip, causing the membrane to split, will bring out still more clearly the different structures of the perfectly clear granular transparent dense membrane, and the enclosed cellular pulp or granular dentinal basis upon its surface. If the tooth be now hardened in alcohol and cut into thin sections, those which are perfectly vertical will show the basement membrane of the small portion of adherent dermis to run up over the tooth as shown in fig. 8. It is exceedingly delicate and perfectly transparent, and when the surfaces of the tissues are cut at all obliquely, it is as invisible as a thin film of glass. Hence it can seldom be followed over the entire surface of even the most perfect section, and it cannot, like the cuticula in a somewhat older tooth, be separated by the use of acid. When a small bit is fortunately torn away with needles, it shows no "cell markings" upon it, but is perfectly smooth.

If a young tooth papilla be treated in this way, the basement membrane will be found quite upon the cellular pulp, a few cells of the enamel membrane frequently adhering to its surface. If the section be teased slightly and the membrane broken, the short outlines of its ends, sometimes standing out alone (Plate 4, fig. 10, *b. m.*), and its clear dense structure appears in marked contrast with the granular underlying pulp.

When the teeth, *in situ*, have been treated with chromic acid for a week or more, as is generally done, and then imbedded, and cut very much at random as is necessarily the case, it is a great chance if any trace of basement membrane can be seen. The enamel cells may appear to lie quite upon the cellular pulp; but if the section be good and sufficiently thin, a close examination will often reveal the membrane upon some part or other of the surface of the pulp.

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* A small tooth of one of the Plagiostomes answers best—say of the thornback.

† At this stage the cuticula, if its formation be begun, is generally brushed off along with the enamel cells.

Fig. 11 represents a section of a tooth papilla a little more advanced. A thin layer of dentinal basis (neither dentine nor enamel) covers the surface of the pulp, and over all is the basement membrane which, in one part, has fortunately been torn away with needles, and is shown to have a very different appearance from the cuticula, which later is developed above it, and can easily be isolated or "raised up" by acid.

In the Plagiostomes the soft dentinal basis under the basement membrane increases in thickness to a considerable depth before the beginning of calcific deposit, and thus affords a better opportunity for the study of the behaviour of the basement membrane, as well as for that of the formation of enamel and dentine, than is met with in mammalian teeth. It frequently becomes two or three times as thick as the future enamel will be before the occurrence of any calcification, which at last begins at the surface and proceeds inwards. The basement membrane of the derma can readily be traced running up over the surface of the young tooth at all stages, while the tissues are still soft. But its fate, after calcification begins, is more difficult to follow; whether it calcifies or remains uncalcified, cannot of course be determined from acid preparations, and from these alone it would seem that thin sections could be made. Experiment has proved, however, that alcoholic preparations of considerably calcified young teeth can, by sacrificing a razor for each section, and coming first upon the soft parts, then upon the hard, be cut into fine sections. These sections throw great light upon the manner of calcification and the relation of the soft and hard parts; they show, too, that after the removal of the cuticula no *un*calcified membrane remains upon the surface of the tooth, but owing to the great refractive power of the calcified portion, it seemed impossible to demonstrate a thin membrane on its margin. The strong appearance of a membrane might still be simply the refractive lines. The persevering and careful use, however, of diluted nitric acid added under the coverslip, brings clearly into view the basement membrane upon the outer surface of the tooth; it is not "raised up," however, as is the case with the cuticula, if it has not been previously removed, and is an altogether different and much more delicate structure than the latter membrane. Fig. 12 is a section prepared in the way just described, showing the basement membrane, and at one place a fragment of the cuticula adhering to it. Teeth far enough developed to show a distinctly differentiated enamel layer with a broad band of dentine underneath, can, with sufficient skill, be cut into the thinnest sections; and, if these are vertical, the basement membrane may always be demonstrated on their surface or under the cuticula if it remain adherent.

Thus the basement membrane may be demonstrated running up from the surface of the dermis over the tooth papilla and young tooth in all its stages. The whole tooth is formed under the basement membrane and the entire growth is from the side of the pulp. The calcification begins at the surface of the tooth, and there is no addition made outside the part already calcified, no "newly formed layer of enamel" appearing as a membrane on the surface, the tooth receiving its additions from the side of the pulp only.

3. *The Origin of the Enamel.*

In its early stage the enamel cannot be distinguished from the dentine. It is formed by a later differentiation of a dentinal basis, which is the same for both dentine and enamel, and the nature of which can be best made out by teasing out, in salt solution, the young still soft tooth of some Plagiostome—say of a skate. It will then be found that the granular basis is formed by the regular arrangement side by side of the slender processes of the odontoblasts (fig. 13), and that they extend right up to the basement membrane, occupying the place of the future enamel as well as of that of the dentine; and it is these regularly arranged processes which cause the striæ often described. If the tooth be quite young, the processes are easily separated and seen in connexion with the cell from which they proceed, and they are frequently branched.

If the tooth be left in ammonium bichromate for a few hours, or even in salt solution, the processes, as well as the cells, can be more readily isolated. Indeed, on simply breaking through the basement membrane many of them escape and will be found outside.

When the tooth becomes a little older, and the *basis* thicker, the processes appear glued together by an intercellular matrix as it were, so that the whole basis appears as one mass, the constituent parts of which are separated with difficulty, and only by prolonged maceration in ammonium bichromate. But, by this time, the deposition of calcareous salts begins; and, soon after, the differentiation of the mineralised portion into dentine and enamel.

The fact that the processes of the odontoblasts extend quite up to the basement membrane, occupying the place of the future enamel, admits of only one view with regard to the origin of the enamel, and explains the existence of tubules which have often been described as extending into it, and continuous with the dentinal tubules.

The mode of calcification and the nature of dentine will be discussed in a future paper.

The results arrived at in this paper may be summed up as follows:—

1. The *cuticula dentis* is formed by the metamorphosis, either in whole or in part, of the enamel cells, which have nothing whatever to do directly with the formation of the enamel. In its early stages

the cuticula has frequently been considered as "the newly-formed layer of enamel" and also as the basement membrane.

2. The basement membrane may be demonstrated upon the surface of the tooth-papilla and upon the tooth in all stages of development. It becomes calcified with the other hard tissue of the tooth and cannot be separated by acid.

3. The enamel, like the dentine, owes its origin to the odontoblasts, the processes of which, in an early stage, may be traced quite up to its outer edge.

EXPLANATION OF FIGURES.

The same letters have been employed to mark corresponding structures in the whole series of figures.

The figures were drawn by the aid of the camera.

b. m.	Basement membrane.
c.	Cuticula dentis.
cal.	Limit of calcification.
cl.	Cleft formed by pressure of coverslip, dissecting needle, &c.
d.	Dentine.
der.	Derma.
di.	Disks.
e.	Enamel.
e. c.	Enamel cells.
e. m.	Enamel membrane.
ep.	Epithelium.
e. l.	External layer of the enamel organs.
e. o.	Enamel organ.
f.	Dentinal basis.
i. l.	Intermediate layer of the enamel organ.
in.	Involution of epithelium (enamel organ).
j.	Jaw.
j. c.	Cartilage of jaw.
n.	Neck of enamel organ.
o.	Odontoblasts.
p.	Pulp.
pr.	Processes of odontoblasts.
r.	Reticulation.
ri.	Ridges.
s.	Space left by shrinkage of tissues.
t. p.	Tooth papilla.
t.	Tubule.
u.	Limit of enamel layer.

Zeiss Oc. III was employed for all the sections. The objective is stated for the different figures.

Figure 1. Membrane lying between enamel and enamel cells of molar of rabbit.

A. *En face* view, the membrane is seen to be not homogeneous, but the reticulation and enclosed disks have a different structure.

B. Side view which shows the reticulation to be elevated into ridges. Two enamel cells and portions of others remain attached, fitting in between the ridges. Obj. $\frac{1}{16}$ immersion.

Fig. 5.

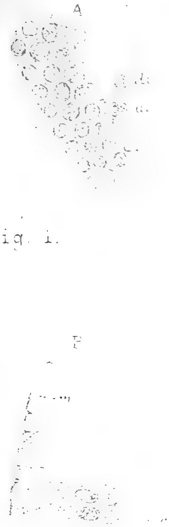

Fig. 1.

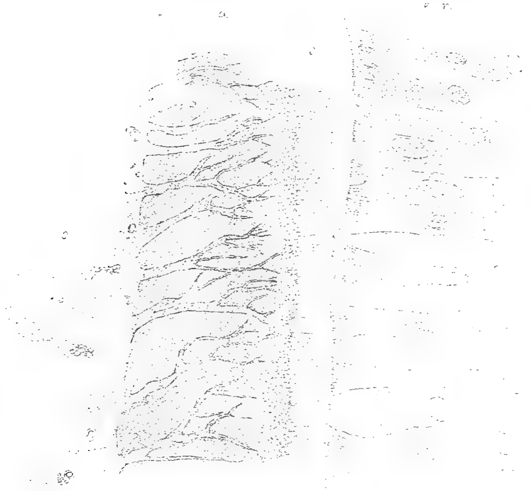

Fig. 3.

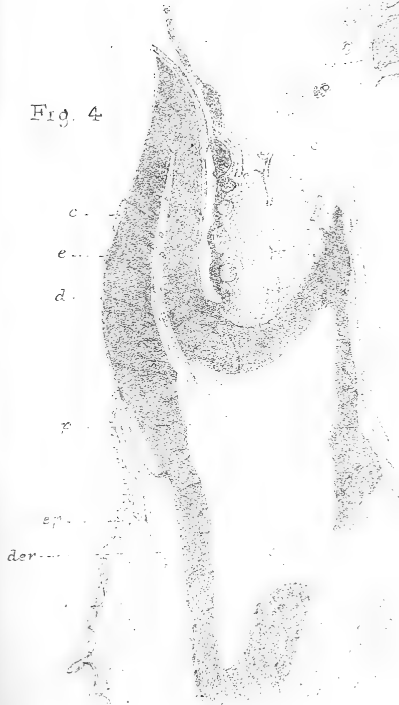

Fig. 4

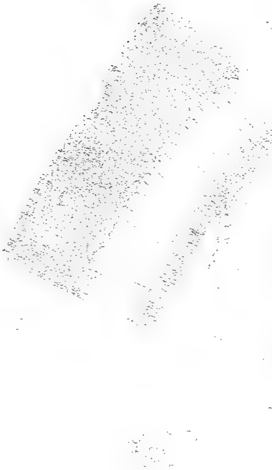

Fig. 2

Fig. 9.

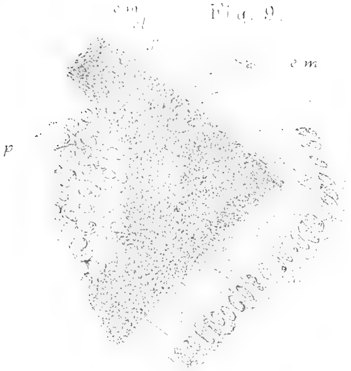

Fig. 6.

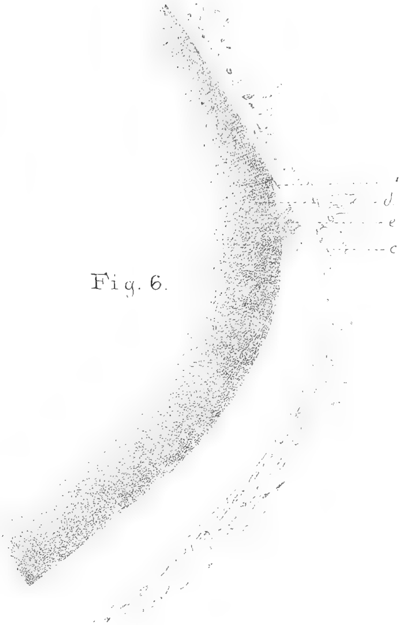

Fig. 7.

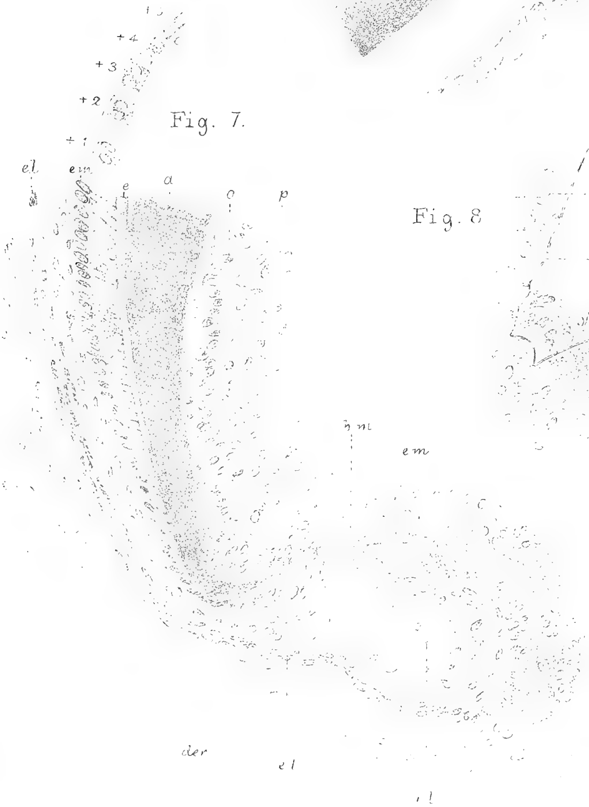
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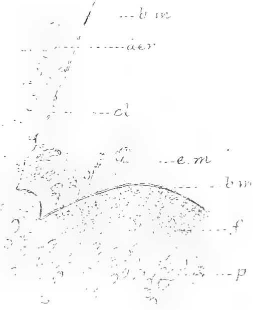
Fig. 8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West Newman & Co. lith.

Fig. 12

Fig. 11

Fig. 13.

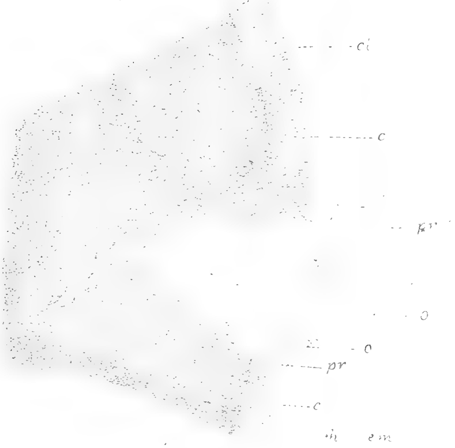
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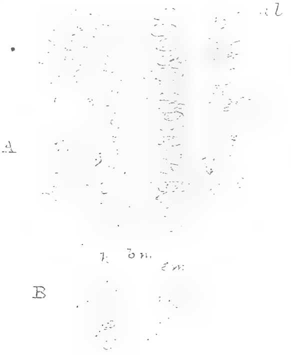
Fig. 10.

Fig. 14.



West Newman & Co. lith.

Figure 2. Part of transverse section of lower incisor of rabbit, showing the membrane (cuticula dentis) lying between enamel and enamel cells.
B. shows an *en face* view of a portion of *c*.
Obj. $\frac{1}{16}$ immersion.

Figure 3. A portion of vertical section of young tooth of an adult *Raia clavata* (thornback), showing the formation of the cuticula by the ends of the enamel cells. Obj. $\frac{1}{16}$ immersion.

Figure 4. Longitudinal section of spine from a fin ray of the thornback, showing the cuticula entire upon the under surface of the spine, with only fragments left upon its upper surface. Obj. 2 inch.

Figure 5. A portion of the cuticula of fig. 4, showing the outlines of the cells and their nuclei. Obj. $\frac{1}{16}$ immersion.

Figure 6. A portion of a transverse section of the lower incisor of rabbit, a short distance below the gum. The enamel extends over the side of the tooth no farther than *u*. A thick layer covered the front of the tooth which, except at one point, has disappeared. Obj. $\frac{1}{16}$ immersion.

Figure 7. A portion of a longitudinal vertical section of the upper small incisor of a rabbit. *e. m.*, 1, 2, 3, 4, 5, are cells of the enamel membrane drawn at intervals, showing their gradual change as they approach the crown of the tooth until, on its exposed portion, they form a homogeneous membrane. Obj. F. Zeiss.

(The following seven sections were from teeth preserved in alcohol, without acid.)

Figure 8. Vertical section of dermis and young tooth of *Raia batis* (skate). The basement membrane upon the derm stands out remarkably clearly, and could be seen running up over the young tooth quite to its apex. Obj. F. Zeiss.

Figure 9. The apex of vertical longitudinal section of tooth of a thornback, showing the basement membrane upon that edge which was cut exactly vertical to the surface of the tooth. No basement membrane can be distinguished upon the other edge, it being cut obliquely to the surface. Obj. F. Zeiss.

Figure 10. A portion of a vertical longitudinal section of a tooth papilla of a young skate. The cellular pulp still extends quite up to the basement membrane, which is more marked as the lines of the cells of the pulp appear to run at right angles to it. Obj. F. Zeiss.

Figure 11. Portion of a vertical section of a tooth papilla of a thornback, in which a thin layer of dentinal basis is seen under the basement membrane, a small piece of which has been fortunately torn away with needles. Obj. F. Zeiss.

Figure 12. Portion of section of a tooth of the skate. Calcification has proceeded from the surface as far as *cal.* The cracks in the calcified portion are produced in cutting. Most of the dentine is still uncalcified. Nitric acid was added under the coverslip and has brought out more clearly the basement membrane, which, however, never in any stage of the tooth can be raised up by acid, as can the cuticula which lies over it, a fragment of which adheres at *c*. Obj. F. Zeiss.

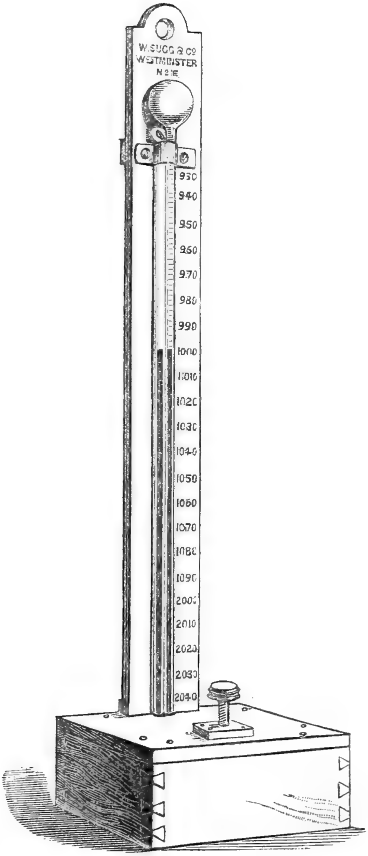
Figure 13. A portion of young tooth of a thornback, teased out in salt solution, showing the dentinal basis to be formed by processes of the odontoblasts arranged closely side by side. Obj. F. Zeiss.

Figure 14. Part of one of a series of vertical sections of tooth papilla of skate. In *B*, cut vertically, the basement membrane *b. m.* is distinctly visible. In *A*, cut more obliquely, it is not seen. Obj. F. Zeiss.

XVIII. "On an Instrument for Correcting Gaseous Volume."

By A. VERNON HARCOURT, M.A., F.R.S. Received June 13, 1882.

This instrument has been devised in order to facilitate the correction of the observed volume of a mass of gas, measured at any common temperature and pressure, to the volume the gas would occupy if measured under standard conditions. A reading of the instrument furnishes a number which serves for the making of this correction, and stands instead of readings of the barometer and of the thermometer, and a reference to a table of the tension of aqueous vapour at different temperatures.


W. SUGG & Co
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The instrument consists of two small glass tubes standing side by side; the one is open above, having been drawn out and bent downwards to exclude dust; the other tube terminates in a bulb, whose capacity is about four and a half times that of the tube. The two tubes are connected below by means of caoutchouc tubing with a small cylinder containing mercury, closed above by a leather cap, which can be pressed down by a button attached to a screw moving in a fixed socket. When the screw and button are lowered the mercury rises in both tubes. The ends of the tubes and the reservoir of mercury are contained in a square box, upon the bottom of which they rest, and whose top carries the socket in which the screw turns. At the back of the box is a wooden upright which supports the tubes. The tube which terminates in a bulb is graduated and figured so as to mark the capacity of the bulb and tube, down to each line of graduation.

In technical measurements of coal gas it is still customary to take for the standard conditions an atmospheric pressure equal to 30 inches of mercury and a temperature of 60° F. The instrument here figured has been made for correcting to these conditions. The capacity of the bulb and stem down to the first line is 3·1 cub. centims., and that of the graduated portion of the stem is 0·7 cub. centim. The bulb and stem have been charged first with a minute drop of water and then with a quantity of air, occupying under standard conditions $3\frac{1}{3}$ cub. centims., the stem below this level being filled with mercury. This volume is marked on the instrument as 1000, the unit taken being $\frac{1}{300}$ cub. centim. The top line of the graduation marks a capacity of 3·1 cub. centims., and is figured 930, this being the smallest volume to which the inclosed air is likely to be reduced by low temperature and high atmospheric pressure. The maximum volume to which the inclosed air is likely to be expanded may be taken at 3·8 cub. centims., and accordingly the lowest line of graduation marked on the stem is 1140.

To use the instrument the pressure of the screw on the mercury is increased or relaxed until the level of the mercury is the same in both tubes. A reading is then made on the graduated stem, and represents the volume occupied at the actual atmospheric pressure and temperature by a mass of air in presence of water which, under standard conditions, occupies a volume 1000. Any volume of gas measured in a gasholder or registered by a meter, under the same conditions, may be corrected to its true volume, under standard conditions, by multiplying by 1000 and dividing by the number read upon the instrument.

When the standard conditions adopted are 0° C. and 760 millims. pressure the bulb is made somewhat larger, so that the 1000 graduation comes near the top of the stem, and the graduations are continued downwards to 1230.

The name proposed for this instrument which serves to correct the measure of a gas, is *aerorthometer.*

XIX. "Sunspots and Terrestrial Phenomena. I. On the Variations of the Daily Range of Atmospheric Temperature, as recorded at the Colaba Observatory, Bombay. II. On the Variations of the Daily Range of the Magnetic Declination, as recorded at the Colaba Observatory, Bombay." By C. CHAMBERS, F.R.S., Superintendent of the Colaba Observatory. Received May 30, 1882.

[Publication deferred.]

XX. "On a Method of Tracing Periodicites in a Series of Observations when the Periods are unknown." By VINAYEK NARAYEU NENE, First Assistant at the Colaba Observatory, Bombay. Communicated by C. CHAMBERS, F.R.S. Received May 30, 1882.

[Publication deferred.]

XXI. "On the Causes of Glacier Motion." By W. R. BROWNE, M.A., late Fellow of Trinity College, Cambridge. Communicated by Professor STOKES, Sec. R.S. Received June 1, 1882.

[Publication deferred.]

XXII. "The Life History of the Ringworm Fungus (*Tricophyton tonsurans*)." By M. MORRIS and Dr. G. C. HENDERSON. Communicated by Professor J. S. BURDON-SANDERSON, M.D., F.R.S. Received June 12, 1882.

[Publication deferred.]

XXIII. "On the Nerves of the Epiglottis." By WILLIAM STIRLING, M.D., Sc.D., Regius Professor of the Institutes of Medicine (Physiology) in the University of Aberdeen, and G. DUFFUS. Communicated by Professor T. H. HUXLEY, F.R.S. Received June 14, 1882.

[Publication deferred.]

XXIV. "On the Action of certain Reagents on Coloured Blood Corpuscles. Part I. Blood Corpuscles of the Frog and Newt." By William Stirling, M.D., Sc.D., Regius Professor of the Institutes of Medicine (Physiology) in the University of Aberdeen. Communicated by Professor T. H. Huxley, F.R.S. Received June 14, 1882.

[Publication deferred.]

The Society adjourned over the Long Vacation to Thursday, November 16th.

Presents, June 15, 1882.

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"Experimental Researches on the Propagation of Heat by Conduction in Bone, Brain-tissue, and Skin." By J. S. Lombard, M.D., formerly Assistant Professor of Physiology in Harvard University. Communicated by Dr. Brown-Séquard, F.R.S. Received October 1. Read November 17, 1881.

Introduction.

The question of the precise degree of the conductivity for heat of the tissues lying between the surface of the brain and the outer surface of the integument is, of course, of the first importance in studying the possible effects on the exterior of the skin of changes of temperature occurring in the superficial layers of the cerebrum; and the question of the degree of conductivity of brain-tissue itself is of great importance with reference to the extent to which propagation through the cerebral mass of thermal changes occurring in a single point or tract of the brain may take place.

Many years ago the writer made a few (not, however, very exact) experiments on the conductibility of bone, which did not lead him to anticipate any serious obstacle in the skull to the outward transmission of heat from the brain. Moreover, the experiments of Professor Tyndall on conduction in elephant's tusk, whalebone, cow's horn, &c., pointed to tissues of this nature as being better conductors than sealing-wax and bees'-wax, on both of which substances the writer had made many experiments, and which he knew would conduct sufficiently well to enable one, with delicate apparatus, to appreciate a slight change of temperature through a thickness of them greater than the average thickness of the skull.

In order to make the theoretical conditions of transmission to the outer surface as unfavourable as could, with any justice, be warranted, the writer selected the conductivity of paraffine as the representative of the conductivity of bone and skin combined, and founded on this basis his line of reasoning respecting the effect of slight variations of the temperature of the surface of the brain on the temperature of the exterior of the skin. But in June, 1880, M. François Franck, in a communication made to the Société de Biologie, gave the results of experiments made by him on the conductivity of bone, skin, and brain-tissue, which placed the whole subject in a new light.* M. Franck stated that a difference of temperature of 1° C. failed to make itself felt at the end of fifteen minutes through 3 millims. of bone, using a thermometer detecting 0°·05 C. With 2° C. difference of temperature a doubtful change of 0°·05 C. was obtained; indeed, it required a difference of 4° C. to effect a change of 0·2°. Using thermo-electric apparatus detecting 0°·01333 C. ($\frac{1}{75}$), M. Franck failed to find any indication of a transmission of heat with a difference of temperature of 1° C. Skin he found to conduct about the same as bone, while on the contrary, through 30 millims. of brain-tissue transmission readily took place.

As it is difficult to conceive of rises of temperature in the brain, due to changes of mental activity, measured by whole degrees Centigrade, M. Franck's experiments on bone and skin, if correct, would peremptorily end all question of the possibility of changes of temperature in the superficial layers of the brain, arising from psychical processes, affecting *directly* the outer surface of the scalp.

So able an experimenter as M. Franck making the above statements, the writer felt himself obliged to go over the whole ground thoroughly, although convinced at the outset that, as regards bone at least, M. Franck was in error. Accordingly, the writer devoted himself for the space of nearly six months entirely to the experimental examination of the conduction of heat in the tissues in question, drawing his results from over 700 experiments picked out from a still larger number. It will be seen that M. Franck is quite correct as regards the comparatively good conductivity of brain-tissue, but in error as concerns the conductivity of bone and skin.*

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* "Gazette Médicale," July 3, 1880.

In approaching this subject, we have at the start, to take into consideration what rises of temperature are likely to occur in the brain as the result of increased mental action of different kinds.

The only *direct* information in our possession concerning the production of heat in the brain during increased cerebral activity is furnished by the well-known admirable experiments of M. Moritz Schiff.† As M. Schiff did not reduce his results to a thermometric standard, we are left wholly in the dark as to the *degree* of the rises of temperature noted by him. It has been rather gratuitously assumed, because M. Schiff did not calculate the thermometric values of the deflections of his galvanometer, that, therefore, these values must have been exceedingly small—too small, in fact, to be easily estimated—and, consequently, that the rises of temperature in the brain were proportionally feeble. But a knowledge of the general nature of the galvanometer and thermo-piles employed by M. Schiff, together with a careful study of the experiments themselves, have failed to prove to the writer that M. Schiff was experimenting with any extraordinary degree of delicacy. To begin with, the electromotive force of the piles employed was not great. Although M. Schiff mentions certain alloys of Rollman, all the results of his experiments on the brain appear to have been obtained with single pairs of either the antimony-bismuth, copper-bismuth, or platinum-German silver combinations. Now the electromotive forces of these combinations may be expressed by 35, 24, and 4·5 respectively, while the electromotive forces of the combinations principally used by the writer are represented by 119·5 and 210. The galvanometer used by M. Schiff was a combination of the principles of the Meyerstein and Wiedemann instruments. These instruments are certainly not superior, even if they are equal, in sensitiveness to the Thomson galvanometer, which the writer has usually employed. The perturbations, arising from external causes, mentioned by M. Schiff, may occur when instruments of the kind are not adjusted to any very great degree of delicacy, and therefore are not necessarily proofs of high sensitiveness. But the principal proof that the galvanometric deflections did not represent very minute values of temperature is to be found in the account of the experiments themselves. It is there stated that with single pairs of German silver and platinum, implanted in corresponding points of the two hemispheres of large dogs, the permanent galvanometric deflections, showing the difference of temperature between the two points, were about 15° of the scale, which was divided into millimetres.* Now it is very unlikely that the temperatures of two points of opposite sides of the brain would, *on an average*, approximate each other nearer than by 0°·03 C.,—this after making full allowance for the good conductivity of brain-tissue. In practice it is difficult to find in the different tissues, unless the points examined are within a centimetre of each other, a nearer approach to equality than the difference just given. Of course, still smaller differences may be met with by accident, but one cannot count upon finding them at a venture. The dogs are specified as *large*, and, indeed, in one place,† M. Schiff gives the distance between the two points examined. In this case each pile was 15 millims. from the longitudinal median line; the two points examined were consequently at least 30 millims. apart. We may assume then that the 15° of the galvanometer did not represent less than 0°·03 C.; therefore 1° of the galvanometer was equal to 0°·002 C. Now the simple odour of food with these animals caused deflections of 6° or 7°, equal to 0°·012 C. to 0°·014 C.,‡ and the mastication of food increased these figures to 12° and 14° of the galvanometer, equal to 0°·024 C. and 0°·028 C. It must, however, be borne in mind that these deflections *did not by any means represent the total rise of temperature, but only the difference of rise between the two points examined.* All M. Schiff's results are in fact *relative*, based on the assumption that one of the two points examined would rise in temperature more than the other. The use of the second pile with him was, in fact, principally for the purpose of keeping the primary deflection of the galvanometer within the field of division on the scale, this pile thus serving as a compensating element. Now if a *difference of rise of temperature* of from 0°·012 C. to 0°·028 C. can be produced in the two hemispheres of the dog by the feeble cerebral action excited by the means given, it is certain that the thermal effects of the active exercise of the intellectual and emotional faculties of man may be estimated in, at least, *tenths* of a degree Centigrade.

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* The question of the *specific heat* of the tissues has been purposely omitted, as nothing definite is known on this important point. Yet the writer is strongly inclined to believe that the differences in the rate of thermal transmission in these tissues are in part owing to differences in their specific heats.

† "Archives de Physiologie," t. III, 1870, p. 6.

In the case of the experiments on fowls, we have further and still stronger proof, both that the apparatus was not excessively delicate, and also that the alterations of temperature were not so very small. In these experiments the thermo-electric arrangement was a small bar of bismuth 4 to 5 millims. long, in the two ends of which copper wires were buried to a depth of 1 millim., thus forming a thermo-electric junction at each end. As the copper wires were embedded in the bismuth to a depth of 1 millim., the two junctions were only from 2 to

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* *Loc. cit.*, pp. 205, 207.

† *Loc. cit.*, p. 211.

‡ *Loc. cit.*, p. 210.

3 millims. asunder. This close proximity of the two junctions must have very greatly diminished the delicacy of the arrangement, as a change of temperature at one junction would speedily be propagated to the other, setting up a reverse current in the latter.* Moreover, considering how good a conductor the brain-tissue is, a slight change of temperature in the point of brain in contact with one junction would very quickly be felt in the point in contact with the other junction, only 4 or 5 millims. of tissue intervening. Again, the galvanometer appears to have been less sensitive in these experiments than in those first cited. Yet M. Schiff obtained deflections of 12° to 14° from the insignificant psychical processes awakened in these animals by the exhibition of coloured papers, &c. It is very evident that, under such adverse circumstances as those specified, the *absolute rise* of temperature must have been considerable to have given any sort of a balance to one point over the other.

Weighing all the evidence, then, there does not appear to the writer, to be the slightest reason why rises of temperature as high as 0°·3 C. should not occur in the brain of man during mental activity; and elevations of 0°·2 C. are certainly admissible; but the results which will be given in this paper are based on values of only 0°·1 C.

In the present experiments, instead of making use of differences of temperature of 1° C., or more, *fractions* of a degree have been employed, as furnishing more conclusive proof of the possibility of the transmission of small differences of temperature, than could be afforded by the mere reasoning from larger to smaller values. We will consider first the apparatus employed, and then the methods of experimenting.

Apparatus Employed.

The instruments employed in testing the conductivity of the tissues under consideration, were as follows:—

First.—Thermo-electric piles of from one to four pairs, composed of the antimony-zinc-cadmium alloy of Professor Moses G. Farmer, joined to bismuth as the other metal. The general construction of these piles has been fully described elsewhere,† and the only point of difference to which special attention need be called here, is, that whereas, in the description referred to, the conducting wires are represented as composed of copper strands, in the present instance they consisted of single fine copper wires 0·011 inch in diameter,—conductors of this size and character being more manageable in packing the pile in paraffine in the manner to be described further on.*

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* See the writer's remarks on reverse currents in piles, in "Regional Temperature of the Head," p. 6. It would have been almost *utterly impossible* to have tested the thermometric values of a pile so constructed,—at least such is the writer's experience.

† See the writer's work "Regional Temperature of the Head," p. 19. The particular alloy referred to above is the one designated "No. 1."

Second.—The writer's rheostat and keys.†

Third.—Sir William Thomson's galvanometer and scale.

The greatest precaution must be taken to guard against the development, in any part of the apparatus, of accidental currents due to external thermal influences. For this reason, not only should every exposed junction of dissimilar—or even *similar*—metals be thoroughly protected with cotton-wool, but also the whole rheostat and the keys should be covered over with several layers of flannel, the plugs and keys being manipulated through a single thickness of the cloth, the other layers being momentarily raised for this purpose, *and only at the very point concerned.* Moreover, besides covering thickly with wool the binding screws of the Thomson galvanometer, the whole brass back of the instrument should be covered with flannel extending over the top and sides of the box containing the coil, and leaving only the glass front exposed.

Methods of Experimenting.

In earlier experiments (1867–68), in the case of bad conductors generally, provided the substances were of sufficient density, a form of apparatus similar to that used by Professor Tyndall in like investigations was employed;‡ but in later, including the present, experiments, the methods adopted were different, and in the present instance were of two kinds, both, however, the same in principle, and differing only in detail.

The fundamental principle of both methods was the determination by means of a thermo-pile applied to one surface of the substance under examination,—say, for example, a piece of bone,—of the rapidity and extent of the change of temperature induced by conduction in this surface by the contact of the opposite surface with a mass of water of a temperature differing in a slight but definite degree from that of the air in the immediate neighbourhood. At the outset, the whole of the piece of bone and the pile, if properly protected, will be at the temperature of the surrounding air; and when contact of one surface of the bone with the water takes place, this surface, assuming the temperature of the water gives rise to a thermal movement across the bone proportional to the difference of temperature between its two surfaces, and as these two surfaces are now respectively at the temperatures of the air and of the water, the movement is proportional to the difference between the latter two temperatures.

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* All possibility of currents caused by vibration of the conducting wires must be guarded against, hence larger wires than those specified, unless flexible like strands, are unsafe.

† *Op. cit.*, p. 22.

‡ "Heat considered as a Mode of Motion," American ed., p. 233.

The first important points of the methods, are, therefore, the determination and regulation of the differences between the temperatures of the air and the water. These differences were determined by thermometers and thermo-piles (the latter being included in a circuit distinct from that of the pile used in testing for conductivity, and having their own galvanometer) placed in and near the water, the thermo-piles giving differences of 0°·02 C. In practice it was found that, with care and patience, a difference of about 0°·125 C. between the air and water could be pretty steadily maintained long enough for the purposes of the experiments. It is, however, as a rule, better to reverse the ordinary order of things, and *to take heat from the bone instead of furnishing heat to it*, that is to say, it is better to have the temperature of the water *lower* by the desired amount than that of the air, than to have it *higher*, for the temperature of the water is more easily maintained at a point differing slightly from the temperature of the air when the former is the lower of the two. If the temperature of the room in which the experiment is made be carefully watched, we may be certain that the temperature of the water will not exceed that of the air; the principal difficulty will be to keep the temperature of the water from falling too much below that of the air, and this end is best attained by withdrawing by suction, through a long tube held in the mouth, a small quantity of the liquid, and then returning it after a longer or shorter stay in the mouth.* A little practice will enable one to graduate, in this simple manner, with great nicety, the temperature of a small mass of water. The amount of water usually employed was about one quart contained in an earthen vessel, exposing no more surface of water to the air than was necessary for the introduction of the different appliances used in the experiments.

We have next to attend to the manner of applying the thermo-pile to the surface of the substance examined, and the precautions necessary in so doing; and here the two methods diverge, the one being applicable to the case of bone, and the other to that of brain and skin. We will consider each method in turn, taking first that which concerns bone.

Bone.

To begin with, the closest possible contact between the face of the pile and the bone must be aimed at. To this end, the surface of the bone is filed smooth, and the face of the pile having been accurately fitted to it, the two are closely and permanently attached to each other by means of a thin layer of shellac varnish applied to the face of the pile and to the surrounding ebonite casing. Firm and steady pressure must be maintained until the shellac is quite dry, as the interposition of minute bubbles of air will be fatal to successful experimenting. The pile and bone thus constitute a single piece.

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* Care must be taken not to alter sensibly the *level* of the water by withdrawing too large an amount, for reasons to be seen further on.

The next step is to isolate, as far as possible, the whole pile from all external thermal influences, except such as act through the piece of bone, or through the conducting wires of the pile. To effect this, the pile is enveloped in its whole length, and beyond to a distance along its conducting wires of several times its length, in layers of fine cotton-wool, which latter are afterwards steeped in melted paraffine. The casing thus formed extends laterally beyond the edges of the surface of the bone to which the face of the pile is attached. The first layer of cotton-wool is applied loosely, and the paraffine is comparatively cool when poured upon the wool. The result of this is that the paraffine does not penetrate very deeply into this first layer, thus leaving a mass of loose wool, next the pile, entangling a certain amount of *air*, and this latter furnishes a strong safeguard against external influences. Of course, care must be taken that the attachment of the face of the pile to the bone be not broken by the heat of the paraffine. When all is complete the whole arrangement consists of a mass of paraffine-soaked cotton-wool some 60 millims. in length, one end of which is terminated by the piece of bone which protrudes from the centre of this end,* while from the other end emerge the conducting wires of the pile, the pile thus forming the core of the mass, and being shut off laterally and at its upper end from the exterior by from 20 to 40 millims. of envelope.

Two narrow strips of pasteboard, bound tightly by means of strips of flannel, on opposite sides of the mass, near its upper end, and brought together and tied so as to form a sort of arch above this end, furnish a *handle* by which the mass can be held vertically, with the exposed bone downwards, by the claw of a horizontal arm working up and down a perpendicular metallic rod fitted into a small but steady stand placed on the table, which supports the vessels containing the water, and the thermometers and thermo-electric appliances used in testing the differences of temperature between the air and the liquid.

Brain and Skin.

The fundamental principle was—as has been said—the same here as in the case of bone, but, as the substances could not with safety be brought into immediate contact with the water, the following special arrangements were adopted: A box of thin pasteboard 50 millims. deep by 85 millims. square was used as a mould, and was filled with melted paraffine. After solidification had taken place, a space was cut out in the centre of the mass, extending from the upper surface to the pasteboard bottom; at and near this latter point the area of the space was just large enough to accommodate the piece of tissue to be tested. The pasteboard bottom under the space was next cut out, and its place supplied by a copper plate less than 0·5 millim. in thickness, which was closely and exactly fitted in, melted paraffine being used on the inside to secure it. The substance to be tested, when in position, therefore rested on the thin copper plate, and was surrounded by paraffine walls.* The pile (enveloped at the end near its face in only a thin layer of paraffine-soaked wool, so as not to touch the surrounding walls of paraffine) was pressed down firmly upon the substance lying on the copper plate, and was kept in position by wedging with cotton-wool the space between its envelope, near the upper end of the latter, and the paraffine walls. The reason for preventing the envelope of the pile near its face from touching the surrounding paraffine walls, is that the latter are, at the bottom of the box, in almost *direct contact with the water;* and as paraffine conducts about as well as the substances tested, a thermal movement might *possibly* take place *directly*, between the face of the pile and the water, through the paraffine walls. The intervention of an *air-space* between the envelopes of the pile and the paraffine wall, not only in the neighbourhood of the face of the pile, but extending to a point far beyond the entire length of the latter, rendered any such thermal movement impossible. Two strips of pasteboard were fitted to the sides of the box, in the same way as in the case of bone. These strips, moreover, served as supports for a mass of cotton-wool, which covered the top of the box, in order to cut off communication between the air imprisoned in the box and the external atmosphere through any chance crevice in the cotton-wool wedges holding the pile in place.

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* One must be sure that the paraffine does not extend down the sides of the piece of bone so as to touch the water when the under surface of the bone is brought in contact with the liquid,—as paraffine will conduct sufficiently well to introduce errors into the results if the above precaution be not taken.

The prepared bone, or the paraffine box containing the piece of brain or skin, having been attached to the claw of the sliding arm mentioned on page 180, by means of the pasteboard strips, is brought over the surface of the water, and then carefully lowered until the under surface of the bone or the copper plate in the bottom of the box is *just immersed, and no more*, in the liquid.† When this is effected the sliding arm is made fast, and the bone or box removed by raising the whole arrangement, as one piece, by means of the perpendicular rod of the stand. The wet surface of the bone or box is next carefully dried with cotton-wool, and protected from external disturbing influences by an enclosure of thick pasteboard placed near the vessel containing the water.

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* In comparing bone with brain and skin, it was found that the interposition of the copper plate had no effect on either the rapidity or the extent of the thermal transmission. This was proved by covering the under surface of a piece of bone, previously tested, with a copper plate of the thickness of that used in the experiments on brain and skin, when it was found that the conductivity remained unchanged.

† The necessity of the caution contained in the note at the bottom of page 180 will now be obvious.

If now, at a given moment, we wish to commence an experiment, we have merely to raise the whole arrangement, as was done when we removed the bone or box, bring the latter over the water, and then *set the stand down again.* As the distance between the water and the substances to be brought in contact with it was previously accurately determined, and the necessary adjustment made with the sliding arm, we may be certain that the proper degree of immersion is ensured. Moreover, as in experiments of this kind a second's time is of importance, and as the above procedure can be timed so as to bring the surface of the bone or box in contact with the water at a given second (and that, too, without the necessity of the observer taking, for a moment, his eyes off the scale of the galvanometer or the timepiece, as the movements necessary can be performed without looking when once their direction and extent are appreciated), it fulfils another important requisite in this portion of the work.* Before adopting this simple procedure the writer made many experiments with more or less complicated apparatus; but all these appliances were, one after another, thrown aside as introducing troublesome, and often dangerous, complications. It must be remembered that the exposed surface of bone, or copper bottom of the box, must be protected, when not immersed, from radiation, possible currents of air, &c., otherwise, thermal exchanges will take place through the exposed surfaces, and —using such delicate means of investigation as we are now treating of —the index of the galvanometer will not be steady for a moment; this being the case, the bone or box cannot be simply suspended, in free air, over the water, to be lowered upon the latter when the appointed time comes; and all attempts to protect them properly, while thus suspended, have led to difficulties, bringing with them, among other evils, delays in the removal of the protections, and, therefore, *errors of time.*

It remains now to describe the manner in which the observations were made.

In the first place, the deflections of the galvanometer were noted regularly every fifteen seconds, commencing from the second at which contact between the bone or copper plate and the water took place, up to six minutes. If, however, as sometimes happened, the first sign of the thermal movement showed itself before the first fifteen seconds had elapsed, of course that particular movement was also noted. After the sixth minute the deflections were noted every half minute or every minute according to the rate of movement of the index of the galvanometer, which was usually much diminished by this time, the permanent thermal condition, or state of thermal equilibrium, being now, as a rule, not very far off. The readings of the thermometers and of the thermo-electric apparatus used in testing the differences of temperature between the air and the water, were noted every half minute. As it was not the rule—even when the greatest care was used—to find the index of the galvanometer at 0° of the scale at the start, it was almost always necessary to add to or subtract from the readings of the thermometers the thermometrical value of the deflection at the moment when the instrument began to show the first sign of the thermal transmission. Thus, suppose the thermometers to show a difference between the air and the water of 0°·125 C. in favour of the former, and the index of the galvanometer to be 5° of its scale on the *cold* side of 0°. If the galvanometer be set to show 1° deflection as equal to 0°·0006742 C., we must deduct 0°·003371 C. ($5 \times 0{\cdot}0006742$) from the 0°·125 C. difference between the air and the water, since the surface of bone or brain or skin in contact with the thermo-pile is already cooler than the air by 0°·003371 C. The *true* thermometric difference between the two surfaces of the substance under examination is, therefore, 0°·121629 C.

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* It is hardly necessary to say that the possibility of currents caused by vibration of the conducting wires of the pile in the movements in question, was fully appreciated, and negatived by direct experiment.

Experiments on Bone.

The bones examined were the skull and long bones of sheep, and the ribs of oxen.

In the experiments on the skull, pieces of various thicknesses and areas were taken from different parts, but the results to be given here were obtained with fresh pieces of the parietal and occipital bones 7·5 millims. in thickness, and 21·5 millims. by 15 millims. in area.

We have three principal points for consideration, namely, as follows:—

(*a.*) The time required for the *first sign* of the change of temperature to show itself through the bone.

(*b.*) The degree of change of temperature produced at certain measured intervals of time.

(*c.*) The maximum of the change of temperature produced, when the *permanent thermal condition* is attained.

Taking the above in the order in which they are set down, we have first to consider the question indicated under the heading "*a.*"

To begin with, the degree of difference of temperature to which the bone was subjected must be taken into account. The average degree of difference of temperature was 0°·129 C., the maximum being 0°·147 C., and the minimum 0°·1136 C. Under these conditions, the average time required for the first appearance through the bone of the thermal change, with the apparatus set to detect 0°·0006742 C., was 28·4 seconds. In 53·333 per cent. of the cases it was 23 seconds; in 26·667 per cent. it was 38 seconds; and in the remaining 20 per cent. it was 30 seconds.

If the results of the different experiments are calculated for 0°·1 C. difference of temperature, on the basis that the time required would be inversely proportional to the degree of difference of temperature, the average time is found to be 37·3 seconds, the maximum and the minimum being respectively 55·86 and 26·29 seconds. The average *rate* of the thermal transmission is, therefore, 1 millim. per 4·9733 seconds, the maximum and the minimum times being, respectively, 7·448 and 3·5053 seconds.

(*b.*) Degree of change of temperature produced at certain measured intervals of time.

We will examine the changes produced at the end of 1 minute and 15 seconds, 2 minutes, 4 minutes, and 6 minutes, respectively, measured from the moment when the bone touched the water. We will take simply the averages and extremes of the changes due to the differences of temperature given under the preceding heading, having first, however, reduced all the results to values representing the effects of 0°·1 C. difference. Table I gives these averages and extremes in both galvanometric and thermometric figures. The galvanometric deflections, it will be seen, indicate the steps towards equalisation of the temperatures of the two surfaces of the bone at the several periods: thus, as 0°·1 C. is equal to 148·316° of the galvanometer,* and as 0°·1 C. represents the difference of temperature between these two surfaces at the start, the steps towards equalisation are measured by the approximation of the figures of the galvanometric degrees to 148·316.

Table I.—Effects of 0°·1 C. difference of temperature through 7·5 millims. of sheep's skull. 1° of galvanometer is equal to 0°·0006742 C.; and 0°·1 C. is equal to 148°·316 of galvanometer.

Time from the moment of contact of bone and water.	Averages.		Maxima.		Minima.	
	Degrees of galvanometer.	Thermometric values.	Degrees of galvanometer.	Thermometric values.	Degrees of galvanometer.	Thermometric values.
At the end of—						
1 min. 15 sec.	23·864°	0·01609° C.	34·916°	0·02354° C.	13·058°	0·00845° C.
2 ,, 0 ,,	54·170	0·03652	74·720	0·05038	30·470	0·02054
4 ,, 0 ,,	88·804	0·05987	115·485	0·07786	40·000	0·02696
6 ,, 0 ,,	116·476	0·07853	135·384	0·09127	68·504	0·04618

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* 1° C. is equal to 1483°·16 of the galvanometer; hence 1° of the galvanometer is equal to 0°·0006742 C.

Percentages of heat transmitted, deduced from the above values.

Times.	Averages.	Maxima.	Minima.
1 min. 15 sec....	16·090 per cent.	23·541 per cent.	8·804 per cent.
2 „ 0 „	36·523 „	50·378 „	20·543 „
4 „ 0 „	59·874 „	77·864 „	26·969 „
6 „ 0 „	78·532 „	91·287 „	46·187 „

This table shows that already by the end of one minute and a quarter the thermal transmission was, on an average, very marked, and that at the end of the sixth minute, 78½ per cent. of the initial difference of temperature had been made up. It will further be seen that these results are widely at variance with those of M. Franck, the latter having failed to obtain, at the end of fifteen minutes, using thermometers detecting 0°·05 C., any indication of conduction through only 3 millims. of bone, with a difference of temperature of 1° C.; while, according to the table, a change of nearly 0°·06 C. was found, at the end of *four* minutes, through 7·5 millims. of bone, with a difference of only 0°·1 C.

(*c*.) The maximum change of temperature produced when the *permanent thermal condition* is attained.

We have under this heading to consider the thermal condition of the bone at the time when the flow of heat through it has settled into a regular and steady movement, in which each cross section of the conductor receives and transmits equal quantities.

We have first to inquire how long a time is usually occupied in the attainment of this condition.

With the differences of temperature specified under the heading (*a*) the time ranged from 9 minutes to 11 minutes 30 seconds, the average of all the times being 9 minutes 53 seconds. In 42·857 per cent. of the cases it was 9 minutes, in 28·572 per cent. it was 10 minutes, in 14·285 per cent. it was 11 minutes 30 seconds, while the remaining 14·286 per cent. was divided equally between 11 minutes and 10 minutes 30 seconds respectively.

Table II gives the effects of the transmission at this period, reduced to values representing 0°·1 C. difference of temperature.

Table II.—*Permanent thermal condition* effected by 0°·1 C. through 7·5 millims. of sheep's skull. 1° of galvanometer is equal to 0°·0006742 C., and 0°·1 C. is equal to 148°·316 of galvanometer.

	Degrees of galvanometer.	Thermometric values.	Percentages of heat transmitted.
Averages	127·431°	0·08591° C.	85·918 per cent.
Maxima.........	138·333	0·09326	93·269 „
Minima.........	104·800	0·07065	70·659 „

We find from the above table that in the permanent thermal state—reached in the majority of cases, as we have just seen, by the tenth minute—the initial difference of temperature of 0°·1 C. between the two surfaces of the bone is, on the average, reduced to 0°·01409 C., nearly 86 per cent. of the excess of heat on the warmer of the two surfaces being now transmitted to the cooler surface.

Experiments on Brain-Tissue.

The brain-tissue used was that of the sheep, and was in a fresh condition. Pretty much the whole of the brain was examined, and blocks of different thicknesses and areas were employed, but the experiments with which we are at present concerned were made on pieces cut from the upper surface of the cerebrum, 7·5 millims. in thickness and of an area of 21·5 millims. by 15 millims., being thus identical in dimensions with the pieces of skull already treated of. A preliminary series of experiments had, however, to be made to determine whether the dura mater opposed any noteworthy barrier to thermal transmission. This question was decided in the negative, it being found that the resistance of the membrane in question was so slight that it could safely be disregarded.

We will examine the results obtained on the pieces of brain in the same manner as was adopted in the case of the skull.

(*a.*) The time required for the *first sign* of the change of temperature to show itself through the piece of brain.

The average degree of difference of temperature to which the brain-tissue was subjected was 0°·13116 C., the maximum being 0°·1513 C. and the minimum being 0°·1202 C. With these differences, the average time elapsing before the first appearance on the upper surface of the piece of tissue of the thermal change (the apparatus having the same delicacy as in the experiments on the skull) was 30·88 seconds. In 44·444 per cent. of the cases it was 23 seconds, in 27·777 per cent.

it was 38 seconds, in 22·223 per cent. it was 30 seconds, and in the remaining 5·556 per cent. it was 53 seconds.

If all the individual results are reduced to values representing 0°·1 C. difference of temperature, the average time becomes 40·49 seconds, the maximum and the minimum being respectively 63·706 and 27·646 seconds. The average *rate* of the thermal movement is, therefore, 1 millim. per 5·3986 seconds, the maximum and the minimum times being respectively 8·4941 and 3·6853 seconds. There appears indeed from these figures to be but little difference at this period between brain and skull.

(*b.*) The degree of change of temperature produced at certain measured intervals of time.

Proceeding in precisely the same manner as in the case of the skull, we arrive at the results set forth in Table III. These results are evidence of the accuracy of M. Franck in attributing a high conducting power (comparatively speaking) to brain-tissue; for the values given in the table approximate closely, especially in the earlier periods, to those contained in Table I for the skull. If we take the differences between the thermometric values at the same periods in the averages of the two tables, we find that the superiority of bone over brain-tissue is represented, even at its greatest, by only a little more than the one-hundredth of a degree Centigrade. The average degree of superiority of the bone over the cerebral tissue in point of conductivity at the different periods will be seen below :—

Table III.—Effects of 0°·1 C. difference of temperature through 7·5 millims. of upper surface of cerebrum of sheep. 1° of galvanometer is equal to 0°·0006742 C.; and 0°·1 C. is equal to 148°·316 of galvanometer.

Time from the moment of contact of copper plate, on which the piece of brain rested, with the water.	Averages.		Maxima.		Minima.	
	Degrees of galvanometer.	Thermometric values.	Degrees of galvanometer.	Thermometric values.	Degrees of galvanometer.	Thermometric values.
At the end of—						
1 min. 15 sec.	21·036°	0·01418° C.	32·445°	0·02187° C.	6·370°	0·00429° C.
2 ,, 0 ,,	42·721	0·02880	59·068	0·03982	19·379	0·01306
4 ,, 0 ,,	74·840	0·05045	103·200	0·06957	32 558	0·02195
6 ,, 0 ,,	99·075	0·06679	136·000	0·09169	49·612	0·03344

Percentages of heat transmitted, deduced from the above values.

Times.	Averages.	Maxima.	Minima.
1 min. 15 sec. . .	14·176 per cent.	21·875 per cent.	4·294 per cent.
2 ,, 0 ,, . .	28·803 ,,	39·825 ,,	13·066 ,,
4 ,, 0 ,, . .	50·459 ,,	69·581 ,,	21·951 ,,
6 ,, 0 ,,	66·799 ,,	91·696 ,,	33·450 ,,

Times.	Thermometric degree of average superiority of skull over brain.	Percentages of degree of average superiority of skull over brain.
1 min. 15 sec.......	0·00191° C.	1·914 per cent.
2 ,, 0 ,, ...	0·00772	7·720 ,,
4 ,, 0 ,, ...	0·00942	9·415 ,,
6 ,, 0 ,, ...	0·01174	11·733 ,,

Comparing the maximum values of the two tables (I and III), it will be noticed that at the end of the sixth minute the brain-tissue *exceeds* the skull by 0°·00042 C.

(*c*.) The maximum change of temperature produced when the *permanent thermal condition* is attained.

First, as to the time required to reach this condition, with the differences of temperature set down under the heading (*a*), the range was from 9 to 11 minutes, the average of all the times being 9 minutes 52·5 seconds. In 50 per cent. of the cases it was 9 minutes, in 37·5 per cent. it was 11 minutes, and in the remaining 12·5 per cent. it was 10 minutes.

In Table IV we have the results of the transmission at this period, reduced, as in the case of the skull, to values representing 0°·1 C. difference of temperature.

Table IV.—*Permanent thermal condition* effected by 0°·1 C., through 7·5 millims. of cerebrum of sheep. 1° of galvanometer is equal to 0°·0006742 C., and 0°·1 C. is equal to 148°·316 of galvanometer.

	Degrees of galvanometer.	Thermometric values.	Percentages of heat transmitted.
Averages..........	113·029°	0·07620 C.	76·208 per cent.
Maxima..........	138·888	0·09364	93·638 ,,
Minima	72·000	0·04854	48·545 ,,

Comparing the above table with Table II, we find that the average difference in the conducting powers of skull and brain-tissue is now reduced to 0°·00971 C., in favour of the bone; but if we take the maximum values, the conductivity of brain-tissue slightly exceeds that of skull, namely, by 0°·00038 C.

Experiments on Skin.

The skin experimented on was fresh sheep's skin; and, in the particular experiments with which we have now to deal, pieces of the shaven scalp 3 millims. in thickness, and of the same area as the pieces of skull and cerebrum already described, were employed.

Following the course adopted with skull and brain-tissue, we have the same points as before to consider.

(*a.*) The time required for the *first sign* of the change of temperature to show itself through the piece of scalp.

The average degree of difference of temperature to which the scalp was subjected was 0°·12957 C., the maximum and the minimum being, respectively, 0°·1645 C. and 0°·125 C. With these differences the time required for the first sign of the change of temperature to manifest itself, on the upper surface of the piece of skin—with the apparatus set, as before, to detect 0°·0006742 C.—was 17·6 seconds. In 60 per cent. of the cases the time was 19 seconds; while the other 40 per cent. was divided equally among 23, 16, 15, and 8 seconds.

Reducing all the results to values representing 0°·1 C. difference of temperature, the average time is found to be 22·88 seconds, the extremes being 29·417 and 10 seconds. The average *rate* of the thermal movement is consequently 1 millim. per 7·6267 seconds, the maximum and the minimum times being, respectively, 9·8057 and 3·3333 seconds. The average rate of the thermal transmission per millimetre for 0°·1 C. difference of temperature appears, therefore, to be lower, at this period, in scalp than in bone or brain-tissue; and the lowest rate in scalp is below the corresponding rates in bone and brain-tissue; but on the other hand, the highest rate is found in scalp, although the degree of superiority is insignificant. In Table V the results obtained on the three tissues, at this period, are brought together for comparison.

(*b.*) The degree of change of temperature produced at certain measured intervals of time.

Table V.—Comparison of times required for the *first sign* of the thermal change to show itself through 7·5 millims. of sheep's skull, 7·5 millims. of sheep's brain, and 3 millims. of sheep's scalp, respectively, with apparatus detecting 0°·0006742 C.

Degrees of difference of temperature to which the several tissues were subjected.

	Bone.	Brain.	Skin.
Averages............	0·1290° C.	0·13116° C.	0·12957° C.
Maxima	0·1470	0·15130	0·16450
Minima.............	0·1136	0·12020	0·12500

With the above differences of temperature the times required for the first appearance through the tissues of the thermal change were as follows:—

	Bone.	Brain.	Skin.
Averages	28·4 seconds.	30·88 seconds.	17·6 seconds.
Maxima	38·0 „	53·00 „	23·0 „
Minima	23·0 „	23·00 „	8·0 „

Percentages of the frequency of occurrence of the different times noted.

Bone.		Brain.		Skin.	
Times.	Percentages.	Times.	Percentages.	Times.	Percentages.
23 seconds	53·333	23 seconds	44·444	19 seconds	60·000
38 „	26·667	38 „	27·777	23 „	10·000
30 „	20·000	30 „	22·223	16 „	10·000
		53 „	5·556	15 „	10·000
				8 „	10·000

Times calculated for 0°·1 C. on the basis that the time required would be inversely proportional to the degree of difference of temperature.

	Bone.	Brain.	Skin.
Averages	37·30 seconds.	40·490 seconds.	22·880 seconds.
Maxima	55·86 „	63·706 „	29·417 „
Minima	26·29 „	27·646 „	10·000 „

Times required to traverse 1 millim. of each of the tissues, calculated for a difference of 0°·1 C.

	Bone.	Brain.	Skin.
Averages	4·9733 seconds.	5·3986 seconds.	7·6267 seconds.
Maxima	7·4480 ,,	8·4941 ,,	9·8057 ,,
Minima	3·5053 ,,	3·6853 ,,	3·3333 ,,

Table VI gives the results obtained at the end on the several times adopted in the preceding tables as bone and brain.

Table VI.—Effects of 0°·1 C. difference of temperature through 3 millims. of sheep's scalp. 1° of galvanometer is equal to 0°·0006742 C.; and 0°·1 C. is equal to 148°·316 of galvanometer.

Time from the moment of contact of copper plate, on which the piece of skin rested, and water.	Averages.		Maxima.		Minima.	
	Degrees of galvanometer.	Thermometric values.	Degrees of galvanometer.	Thermometric values.	Degrees of galvanometer.	Thermometric values.
At the end of—						
1 min. 15 sec.	17·191°	0·01159° C.	21·120°	0·01424° C.	10·576°	0·00713° C.
2 ,, 0 ,,	31·241	0·02106	38·808	0·02616	20·898	0·01409
4 ,, 0 ,,	59·208	0·03992	83·952	0·05660	35·861	0·02417
6 ,, 0 ,,	80·766	0·05445	96·492	0·06505	63·884	0·04307

Percentages of heat transmitted, deduced from the above values.

Times.	Averages.	Maxima.	Minima.
1 min. 15 sec.	11·597 per cent.	14·239 per cent.	7·137 per cent.
2 ,, 0 ,, ...	21·063 ,,	26·165 ,,	14·090 ,,
4 ,, 0 ,, ...	39·921 ,,	56·603 ,,	24·179 ,,
6 ,, 0 ,, ...	54·452 ,,	65·058 ,,	43·070 ,,

It will, at once, be evident, that, although the pieces of skull and of cerebrum are two and a-half times thicker than the pieces of skin, yet the amount of heat transmitted by the latter is considerably less than the amount transmitted by the former, with the exception, that the minimum values of scalp are higher than the corresponding values of brain-tissue, and approach somewhat closely to those of skull.

If we should apply the well-known physical calculations of Fourier and others, and through them seek to determine the changes of temperature which would exist if the piece of skin were increased in thickness to 7·5 millims., the inferiority of the tissue in conducting power compared with bone and brain-tissue, would become much more striking;[*] but unfortunately, not only theory—based upon the lack of homogeneity in these structures—but also a large number of direct experiments made by the writer, show that such calculations are not to be relied upon. In the case alone of the hard tissue of bone, it has sometimes happened that the results of the mathematical calculations and those of the experiment have partially agreed. We cannot, then, with any certainty, reason from one thickness of bone, brain, or skin to another. To have reduced the bone to 3 millims. in thickness to correspond with the skin, would have entailed serious risks of error in the method of experimenting adopted.[†] The thickness of bone chosen was a natural thickness of the skull often found in the animal experimented on, and the same is true of the thickness of the scalp.

(*c.*) The maximum change of temperature produced, when the *permanent thermal condition* is attained.

With the differences of temperature given under the heading (*a*), the permanent thermal condition was reached in a time ranging from 11 to 15 minutes, the average being 12 minutes 15 seconds. In 50 per cent. of the cases the time was 11 minutes, and the other 50 per cent. was divided equally between 12 and 15 minutes.

In Table VII we see the effects of the thermal movement at this stage, reduced as before to the basis of 0°·1 C. difference of temperature.

Table VII.—*Permanent thermal condition* effected by 0°·1 C. through 3 millims. of sheep's scalp. 1° of galvanometer is equal to 0°·0006742 C.; and 0°·1 C. is equal to 148°·316 of galvonometer.

	Degrees of galvanometer.	Thermometric values.	Percentages of heat transmitted.
Averages	100·155°	0·06751° C.	67·514 per cent.
Maxima	117·480	0·07920	79·209 ,,
Minima	82·104	0·05535	55·354 ,,

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[*] The application of these formulæ *sweeps away the whole of Table VI*, as according to them, even at the end of the sixth minute no sign of the transmission would be found through 7·5 millims. of scalp.

[†] By exchanges between the face of the pile and the water through the paraffine envelope (see note, p. 181), which latter would, with the above thickness of bone, be in dangerous proximity to the liquid.

Placing the above beside Tables II and IV, even leaving out the question of relative thickness, the inferior conducting power of skin, compared with bone and cerebral tissue, is again manifest, although the degree of this inferiority is diminished. Thus, taking the averages at the end of the sixth minute, the skin falls below bone by 24·08 per cent., and below brain-tissue by 12·347 per cent.; while now these differences are reduced, respectively, to 18·404 per cent., and 8·694 per cent. If we take the maximum values, the skin is inferior to bone (the maximum value of the latter being a trifle lower than that of brain-tissue) by 26·229 per cent. at the end of the sixth minute, and by 14·06 per cent. in the permanent thermal condition. With regard to the minimum values, they are now, as at former periods, higher in skin than in brain-tissue.

Conduction in Bone and Skin combined.

Let us now suppose the 3 millims. of scalp to be lying upon the 7·5 millims. of bone, as in life, and a rise of temperature of 0°·1 C. to occur on the cerebral surface beneath. We have seen that the dura mater offers no appreciable resistance, and have, therefore, simply to deal with the compound conductor of bone and skin. We will first estimate how long a time would elapse after the rise of temperature in the brain before 0°·0006742 C. difference would be found on the outer surface. Now it has been shown that the average time required for 0°·1 C. to traverse the bone is 37·3 seconds, while the average time required for the same difference of temperature to traverse the skin is 22·88 seconds; the total time would therefore, be 60·18 seconds, the shortest time would be 36·39 seconds, and the longest time 85·277 seconds.

Next, with regard to the amount of heat which would be transmitted through the compound conductor. Looking at Table I we see that the bone has transmitted, at the end of 1 minute 15 seconds, 16·09 per cent. of the heat received, and from Table VI we learn that, during the same time the skin has transmitted 11·597 per cent.;* therefore, the skin receiving from the bone 16·09 per cent. of the original amount of heat would transmit 11·597 per cent. of these receipts, or 1·86395 per cent. of the original amount; hence the change of temperature observed on the outer surface of the scalp, at this period, would be 0°·001866 C. Table VIII gives the results, for the several periods of time, deduced in the above manner from Tables I and VI. These results show that, in spite of the decided resistance introduced by the skin, there would not be the slightest difficulty in detecting, with delicate apparatus, at an early period, on the outer surface of the scalp, a change of 0°·1 C. on the surface of the brain, in the animal in question.

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* Averages.

Table VIII.—Effects of 0°·1 C. difference of temperature through 7·5 millims. of sheep's skull and 3 millims. of sheep's scalp, *taken together.* 1° of galvanometer is equal to 0°·0006742 C.; and 0°·1 C. is equal to 148°·316 of galvanometer.

Times.	Averages.		Maxima.		Minima.	
	Degrees of galvanometer.	Thermometric values.	Degrees of galvanometer.	Thermometric values.	Degrees of galvanometer.	Thermometric values.
At the end of—						
1 min. 15 sec.	2·767°	·001866° C.	4·971°	·003352° C.	0·931°	·000628° C.
2 „ 0 „	11·409	·007692	19·550	·013181	4·293	·002894
4 „ 0 „	35·450	·023902	65·367	·044073	9·671	·006520
6 „ 0 „	63·424	·042762	88·083	·059389	29·504	·019892

Percentages of heat transmitted.

Times.	Averages.	Maxima.	Minima.
1 min. 15 sec.	1·866 per cent.	3·352 per cent.	0·628 per cent.
2 „ 0 „	7·692 „	13·181 „	2·894 „
4 „ 0 „	23·902 „	44·073 „	6·520 „
6 „ 0 „	42·762 „	59·389 „	19·892 „

Coming to the permanent thermal condition of the compound conductor, we can estimate, in the same manner as we have just done, from the separate tables for bone and skin, the amount of heat which would be transmitted when this condition is reached. Table IX gives the results of these estimates. Here, again, we have evidence that—although diminished—the external manifestations of 0°·1 C. change at the cerebral surface would still be amply great to admit of detection by much coarser instruments than those we are employing. Supposing the rise of temperature at the cerebral surface to be only 0°·01 C., instead of 0°·1 C., it would still be plainly visible at the exterior at the end of the fourth minute; for the percentage of transmission at this period would give a galvanometric deflection of 3°·545, equal to 0°·00239 C., while when the permanent thermal condition was attained, the deflection would be 8°·603, equal to 0°·0058 C. But, as was stated in the introduction, there is no reason whatever why rises of temperature of 0°·2 C., and even 0°·3 C. may not occur in the brain of man, and perhaps in the brains of other of the higher animals, during intellectual and emotional activity, with, consequently, decidedly greater external manifestations than those given in our calculations.

Table IX.—*Permanent thermal condition* effected by 0°·1 C. through 7·5 millims. of sheep's skull and 3 millims. of sheep's scalp, *taken together.* 1° of galvanometer is equal to 0°·0006742 C.; and 0°·1 is equal to 148°·316 of galvanometer.

	Degrees of galvanometer.	Thermometric values.	Percentages of heat transmitted.
Averages	86·033°	0·058006° C.	58·006 per cent.
Maxima	119·572	0·073877	73·877 ,,
Minima..........	58·010	0·039112	39·112 ,,

With regard to the effect of the blood circulating between the surface of the brain and the outer surface of the skin, the only way in which this liquid could check the outward thermal propagation would be by virtue of its specific heat. The writer has considered this question at some length in the work already cited,* and he sees no reason now to depart from the line of argument there followed. If, as was there done, we allow a loss of 50 per cent. of the initial rise of temperature to satisfy the capacity for heat of the blood (and we are really not warranted in granting such a loss) our 0°·1 C.—now reduced to 0°·05 C.—would still show itself at the outer surface, at the end of the second minute, by a galvanometric deflection of 5°·704, equal to 0°·003846 C.

We have next to see how far the good conductivity of brain-tissue would act to prevent localisation at the outer surface of the scalp of changes of temperature in a narrowly circumscribed area of the cerebral surface.

Imagine, as before, a point of the cerebral surface to have its temperature raised 0°·1 C. Now, setting out from this point, the excess of heat would be transmitted to points in the surrounding cerebral mass situated at a distance of 7·5 millims., in the proportions shown in Tables III and IV. What the transmission to a point of the external surface situated directly over the focus of heat would be we have just seen. We have, then, merely to take the temperatures contained in Tables III and IV, and using the percentages of transmission through skull and scalp combined, given in Tables VIII and IX, to calculate the temperatures which would be found at a point of the outer surface lying over the point of cerebral surface situated 7·5 millims. from the focus of heat. For example, Table III shows us that, at the end of the sixth minute, a point of the brain, situated 7·5 millims. from another point heated 0°·1 C., would have its own temperature raised, by conduction, 0°·06679 C., and Table VIII shows us that the transmission through skull and scalp combined (which would, of course, be proceeding coincidently) is, at this time, 42·762 per cent.; hence the temperature of the outer surface would be 0°·02856 C. Tables X and XI show the effects of this indirect transmission to the outer surface.

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* *Op. cit.*, pp. 115, 118.

Table X.—Effects produced through 7·5 millims. of sheep's skull and 3 millims. of sheep's scalp, taken together, lying over a point of cerebral surface 7·5 millims. distant from another point of this same surface, the temperature of which latter point is raised 0°·1 C. The results are calculated from Tables III and VIII. This table is for comparison with Table VIII where the effects of the *direct* transmission from the heated point are given. 1° of galvanometer is equal to 0°·0006742 C.; and 0°·1 C. is equal to 148°·316 of galvanometer.

Times.	Averages.		Maxima.		Minima.	
	Degrees of galvanometer.	Thermometric values.	Degrees of galvanometer.	Thermometric values.	Degrees of galvanometer.	Thermometric values.
At the end of—						
1 min. 15 sec.	0·392°	·000264° C.	1·087°	·000733° C.	0·040°	·000027° C.
2 „ 0 „	4·873	·003286	7·786	·005249	0·560	·000378
4 „ 0 „	17·888	·012060	45·483	·030664	2·122	·001430
6 „ 0 „	42·360	·028560	70·769	·047712	9·868	·006633

Percentage of heat transmitted.

Times.	Averages.	Maxima.	Minima.
1 min. 15 sec.	0·264 per cent.	0·733 per cent.	0·027 per cent.
2 „ 0 „	3·286 „	5·249 „	0·378 „
4 „ 0 „	12·060 „	30·664 „	1·430 „
6 „ 0 „	28·560 „	47·712 „	6·633 „

Table XI.—Permanent thermal condition effected through 7·5 millims. of sheep's skull and 3 millims. of sheep's scalp, taken together, lying over a point of cerebral surface 7·5 millims. distant from another point of this same surface, the temperature of which latter point is raised 0°·1 C. The results are calculated from Tables IV and IX. This Table is for comparison with Table IX, where the effects of the *direct* transmission from the heated point are given. 1° of galvanometer is equal to 0°·0006742 C.; and 0°·1 C. is equal to 148°·316 of galvanometer.

	Degrees of galvanometer.	Thermometric values.	Percentages of heat transmitted.
Averages	65·563°	0·044202° C.	44·202 per cent.
Maxima	102·606	0·070176	70·176 „
Minima..........	28·160	0·018985	18·985 „

Plainly, if it were a question of mere conduction alone, and if the skull and skin at the several points were of equal thickness, and possessed of the same conductivity, it would be easy to locate on the outer surface, within a radius of 7·5 millims., a change of 0°·1 C. occurring on the cerebral surface.

The following are the differences of temperature in favour of the point of surface lying directly over the focus of heat, which would be found under the circumstances we are considering:—

Times.	Average differences of temperature.	Permanent thermal condition.	
			Differences of temperature.
1 min. 15 sec....	0·001602° C.	Average	0·013804° C.
2 „ 0 „ ...	0·004406	Maximum	0·003701
4 „ 0 „ ...	0·011842	Minimum*......	0·020127
6 „ 0 „ ...	0·014202		

But, in truth, in the case of the tissues concerned, we are not, in the first place, dealing with simple homogeneous conductors of uniform thicknesses. Even within the narrow area specified, the bone or skin may exhibit decided differences of conductivity, due to slight variations of structure or composition. That this may be the case, the writer has over and over again proved by direct experiment. As the propagation of heat by conduction is not *rectilinear*, a slight alteration of texture or composition might easily deflect the path of transmission in such a way as to wholly change the relative temperatures of the outer surface which we have given. Differences of thickness, also small, but sufficient to overthrow our calculations—may exist. Lastly, the circulation of the blood, already alluded to, although incapable of checking the outward transmission, might yet, within such narrow limits, bring about a confusion in the external manifestations of the interior change of temperature. It is only when areas of much greater dimensions—for instance, of 50 or 60 millims. square—are taken, that we can look with any degree of confidence to the relative external temperatures as furnishing a key to the relative temperatures of the underlying tracts of cerebral surface.* Moreover, in increased mental activity—whatever may be its kind—the change of temperature on the outer surface of the head is of widespread extent, and not confined to such limited areas as those on which our calculations are based.

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* It will be noticed that the *least* difference is found with the *maximum* of transmission, and the *greatest* difference with the *minimum* of transmission.

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* See the writer's "Regional Temperature of the Head," pp. 119 and 209.

"On the Variation of the Electrical Resistance of Glass with Temperature, Density, and Chemical Composition." By THOMAS GRAY, B.Sc., F.R.S.E. Communicated by Professor Sir WILLIAM THOMSON, F.R.S. Received December 28, 1881. Read January 12, 1882.*

The following paper is a description of the methods adopted, and of the results obtained, in a series of experiments on the specific resistance of glass. These experiments were performed in the Physical Laboratory of the Imperial College of Engineering, Tokio, Japan.

An account of some preliminary experiments on this subject was communicated by the author of this paper to the "Philosophical Magazine" for October, 1880. In that paper attention was specially directed to the change of resistance with change of temperature, and to an apparently permanent change in electric quality which the glass underwent when subjected to a high temperature. Subsequent experiments have served to confirm the results there given, but show that if the glass be newly made very little, if any, permanent change is brought about by heating.

In the experiments just referred to a current of electricity was kept flowing either continuously or at short intervals during the heating. As this might produce effects which would not be caused by heating alone, it was thought desirable to test one or two specimens for resistance at as low a temperature as possible, and then again, after the glass had been heated to between 200° and 300° C., and cooled to the same temperature. Experiments performed in this way have shown an exactly similar change to that previously obtained. It appears, therefore, that the change previously observed was due to heating.

The fact that the permanent change produced by heating to a high temperature was markedly greater in specimens of old than in specimens of new glass, rendered it probable that the change was due to some previous change in the opposite direction, which goes on slowly at the ordinary temperature. In order to put this conjecture to the test of experiment, advantage was taken of several specimens of newly-manufactured glass which had just been obtained from the Government Glass Works, Shinagawa, Tokio. The results of tests made on three specimens of that glass are given in the following table. The first two specimens were lime glass, while the third was a white semi-opaque flint glass, containing arsenic. In the first column the number of the specimen is written; in the second the resistance in ohms between two opposite faces of a cubic centimetre; in the third, the temperature at which the resistance was measured; in the fourth, the density of the glass; and in the fifth, the date at which the resistance was measured.

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* For abstract see *ante*, vol. 33, p. 256.

No. of specimen.	Specific resistance in ohms per cub. centim.	Temperature.	Density.	Date.
1	146×10^{10}	40° C.	2·57	May 3, 1880.
	122×10^{10}	"	"	December 9, 1880.
2	24×10^{10}	40° C.	2·53	May 3, 1880.
	17×10^{10}	"	"	December 10, 1880.
	12×10^{10}	"	"	May 3, 1881.
3	41×10^{11}	140° C.	3·07	May 17, 1880.
	17×10^{11}	"	"	November 17, 1880.

These results show a very considerable increase of conductivity with age, and also show a marked difference in the variation of different specimens. The number of experiments is not sufficient to give much information regarding this time change, but the fact that they give evidence that such a change takes place seems to warrant the publication of these preliminary results.

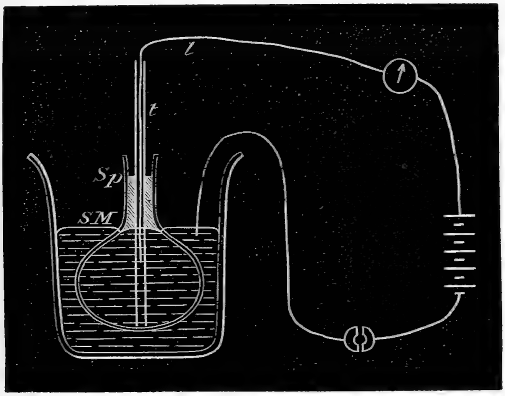
The measurements of resistance described in this paper were, like those in the previous paper above referred to, for the most part made by means of an astatic galvanometer of high resistance and great sensibility. The galvanometer used had an internal resistance of 10,000 ohms, and one Daniell's element produced a deflection of one division when a resistance of about 10^{11} ohms was in the circuit. The great advantage of the galvanometer over the electrometer method of measurement is its simplicity; the deflection being independent of the capacity of the circuit, provided no change is taking place in that capacity. In many cases, however, the resistance of glass at low temperatures cannot be measured by the galvanometer, and in these cases, the most convenient instrument is a Thomson's quadrant electrometer.

The method adopted in the galvanometer measurements was that of direct deflection, the current being produced by fifty Daniell's elements, placed on a table well insulated with ebonite supports, kept dry by being enclosed in boxes containing sulphuric acid. The main difficulty in this method is to ensure absence of leakage currents through the galvanometer. The test used for the absence of such currents was to insulate the electrode of the inside coating of the glass vessel, and then close the key. If there was no deflection, it was assumed that the circuit was sufficiently insulated.

Several measurements were made by means of the quadrant electrometer, and in that case the method adopted was to connect one coating of the glass, one pair of quadrants, and the case of the electrometer to earth; while the other coating of the glass and the remaining pair of quadrants were connected together but insulated. The resistance was then calculated from the capacity of the glass vessel and electrometer quadrant, and the rate of loss of charge.

The conducting coatings for the glass were generally made by partly filling the vessel to be experimented on with mercury, and then immersing it in another vessel of mercury until the surface of the mercury inside and outside the vessel was at the same level. In order to avoid leakage over the sides of the vessel, it and the mercury were made thoroughly dry by heating, and when sufficiently cooled, a coating of paraffin was run over the surface of the mercury and the vessel. Through this coating of paraffin a fine glass tube, well dried and paraffined, was passed, thus furnishing at the same time a passage, and more thorough insulation against surface leakage for the electrode which made contact with the mercury. This explanation will be more readily understood by the aid of fig. 1, which shows the arrangement for measuring the resistance of a glass globe, the galvanometer, battery, and key being shown symbolically. In the figure SM represents the surface of the mercury, S*p* the surface of the paraffin, and *t* the fine tube through which the electrode, *l*, passes. The tube, *t*, and the neck of the globe were in such a case coated with paraffin.

FIG. 1.



The precautions against leakage here described are more necessary when the resistance at ordinary temperature is to be measured, but even in other cases it was found advisable to begin with this, and simply allow the paraffin to evaporate at high temperatures. The surface of the hot glass remained afterwards perfectly dry.

Sulphuric acid was sometimes used instead of mercury, and answers perfectly if the temperature does not require to be high. If, however, the temperature requires to be raised until the acid evaporates, it becomes extremely disagreeable. The acid has the advantage that it keeps the vessel dry, and hence is to be preferred for low temperature measurements.

Abridged tables of results for a few characteristic specimens are annexed, and serve to illustrate the very wide range of resistance which may be obtained by using different specimens of glass. The variation with temperature of several specimens is illustrated by means of curves. These curves only show the variation with temperature through a small range, as it was found almost impossible to include both a number of curves and long range of temperature in the same diagram.

It will be observed on examining these curves that the rate of variation with temperature is very nearly the same, not only for different specimens of the same kind of glass, but for all the kinds of glass there figured. Other specimens, not included in this diagram, gave a very similar variation. On an average it may be said that the specific resistance of glass is halved for every 8°·5 C. rise of temperature.*

In the tables of results the density of each specimen is recorded, and in some cases the chemical composition also. The chemical analyses were performed in the Chemical Laboratory of the Imperial College of Engineering, Tokio, by Messrs. Fujii and Shimidzu, under the superintendence of Dr. Edward Divers, to whom the author is much indebted for the great interest he has taken, and assistance he has given, in the carrying out of these experiments.

It is very interesting to notice how very closely a change of density in flint glass agrees with a change of electrical resistance, and also that the electrical resistance of this kind of glass increased regularly until the density reached that point at which the composition of the glass was almost exactly that required for a trisilicate of lead, potash, and silica. The very high density of lead oxide causes the density of the glass to be an indication of the quantity of lead present, and hence the density in this case serves as a guide to the electrical quality of the glass. A specimen of glass containing too much lead for a pure silicate has not yet been experimented on, but the result of such an experiment would be of great interest in furnishing evidence as to whether purity of chemical composition and high electrical resistance go together.

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* Note added April 26, 1882.—Although the fall of resistance with rise of temperature generally follows very nearly the logarithmic law, the results show variations from that law which I am not yet able to explain. The resistance at high temperatures is generally higher than would be inferred from the resistance and rate of variation at low temperatures. It is remarkable that specimens which had a high resistance gave results more nearly in agreement with the logarithmic law than specimens of comparatively low resistance.

The resistances quoted in the tables are those calculated from observations after one minute's electrification, the direction of the current being alternately in opposite directions, and only allowed to flow for about one minute at each observation. The method of observation was thus similar to that described as "the first method" in my paper in the "Philosophical Magazine" above referred to. (See "Phil. Mag.," October, 1880, page 227.)

When we turn to lime glass, however, we find that the density is no guide to the electrical quality. Specimens having nearly the same density vary enormously as to their electrical resistance. This, however, is to be expected, as the density may change but little, even when the chemical composition is greatly altered. Lime glass generally contains both soda and potash, and the ratio of these two bases may influence considerably the density, while the glass remaining a good glass the electrical conductivity may not be much affected.

So far as the results of chemical composition go, however, it appears that in the case of lime glass also, a glass which would be pronounced good from a chemical point of view is also relatively good from an electrical point of view. On the other hand a glass which would be pronounced bad chemically is also bad electrically.

In the following tables the resistance at various temperatures of six specimens of lime glass and two specimens of lead glass are given. The first column contains the temperature, the second the resistance in ohms of a cubic centimetre, the third the density, and the fourth the chemical composition in those cases where it was determined.

Specimen I. (Bohemian glass tubing.)

60° C.	605×10^{11}	2·43
100	20×10^{11}	
130	20×10^{10}	
160	24×10^{9}	
174	87×10^{8}	

Specimen II. (Test-tube.)

37° C.	229×10^{10}	2·458
59	306×10^{9}	
73	612×10^{8}	
101	56×10^{8}	
131	62×10^{7}	

Specimen III. (Japanese lime glass tubing.)

10° C.	670×10^{10}	2·55	Silica	61·3
30	199×10^{10}		Potash	22·9
52	300×10^{9}		Lime, &c., by diff.	15·8
75	450×10^{8}			——
85	220×10^{8}			100·0

Specimen IV. (Japanese lime glass tubing.)

35° C.	113×10^{11}	2·499	Silica	57·2
55	25×10^{11}		Potash	21·1
75	61×10^{10}		Lime, &c., by diff.	16·7
85	26×10^{10}			
95	12×10^{10}			100·0

The analyses of the last two specimens are only approximate, having been made previous to the electrical experiments, and for a different purpose. The composition differs very widely from that which is required for a pure silicate, and the electrical resistance is also found to be very low.

Specimen V. (French flask.)

45° C.	327×10^{10}	2·533	Silica	70·05
55	133×10^{10}		Lime	10·33
65	509×10^{9}		Lead oxide	2·70
75	204×10^{9}		Soda	14·32
86	812×10^{8}		Potash	1·44
95	391×10^{8}		Magnesia	0·10
108	133×10^{8}		Alumina, iron oxide, manganese oxide	1·45
117	707×10^{7}			
				100·39

With regard to this specimen, Dr. Divers writes as follows:—

"This seems to be a soda lime glass mixed with a little potash lead glass; the latter having been thrown in as cullet. Assuming this to be the case, we will have approximately—

Potash lead glass	8·7
Soda ,,	91·3
	100·0

The soda lime glass has then the composition—

Silica	72·7
Lime	11·4
Soda	15·9
	100·0

Apparently the best glass has the formula—

$$\mathrm{CaO,Na_2O,}\underset{6}{\underline{\mathrm{SiO_2}}}=\left\{\begin{array}{lr}\text{Silica......} & 75\cdot3\\ \text{Lime......} & 11\cdot7\\ \text{Soda......} & 13\cdot0\\ \hline & 100\cdot0\end{array}\right.$$

"If this is deviated from an increase in the proportion of soda to lime requires a considerable increase in the proportion of silica to base. The flask is therefore defective, for not only is the soda in excess to the lime, but the silica is deficient. I calculate that from 20 to 25 parts of silica should be added to 100 of that glass to counteract the excess of soda. Such a glass would be—

Silica	77 ·5
Lime........................	9 ·5
Soda	13·0

"However, too little is yet known of the relations of composition to quality of glass to admit of positive statement.

"The empirical formula $x\overline{\mathrm{CaO,SiO_2}} + y\overline{\mathrm{Na_2O,}\underset{5}{\underline{\mathrm{SiO_2}}}}$ seems to me to be a tolerably accurate expression of the various kinds of good glass, provided x and y are not very different from one another. When equal, the glass is certainly excellent."

Specimen VI. (Bohemian beaker.)

66° C.	497×10^{11}	2 ·587	Silica	75 ·65
88	828×10^{10}	..	Lime..............	8 ·48
110	138×10^{10}	..	Potash	7 ·92
132	230×10^{9}	..	Soda..............	6 ·92
150	540×10^{8}	..	Magnesia	0 ·36
170	147×10^{8}	..	Alumina, iron and manganese oxides.	0 ·70
193	308×10^{7}	..		
				100 ·03

Assuming the formula $\mathrm{K_2O,CaO}\underset{6}{\underline{\mathrm{SiO_2}}} + \mathrm{Na_2O,CaO,}\underset{6}{\underline{\mathrm{SiO_2}}}$, as giving the best composition, we should have—

Potash	18 ·4	Soda............	13 ·0
Lime.	11 ·0	Lime	11 ·7
Silica...........	70 ·6	Silica..........	75 ·3

The mean of which would give—

Potash	9·2
Soda	6·5
Lime	11·3
Silica	73·0

The alkali is therefore slightly in excess, but to compensate that, there is an excess of silica, the result being a very good glass, both chemically and electrically.

Specimen VII. (Arsenic-enamel glass.)

49° C.	140×10^{13} ?	3·07	Silica		54·2
105	230×10^{11}		Lead oxide		23·9
115	101×10^{11}		Potash	17·5	*10·5
125	45×10^{11}		Soda		* 7·0
135	22×10^{11}		Lime		0·3
			Magnesia		0·2
			Iron and manganese oxide and alumina		0·4
			Arsenic oxide by diff.		3·5
					100·0

In this glass we have an excess or alkali for the lead oxide, and a deficiency of silica; the composition is rendered complicated, however, by the presence of the arsenic.

Specimen VIII. (Thomson's electrometer jar.)

100° C.	206×10^{12}	3·172	Silica	55·18
120	468×10^{11}		Lead oxide	31·01
140	106×10^{11}		Potash	13·28
160	245×10^{10}		Lime	0·35
180	56×10^{10}		Magnesia	0·06
200	12×10^{10}		Alumina, iron and manganese oxides	0·67
				100·55

The formula $\mathrm{PbO,K_2O,}\underset{6}{\underline{\mathrm{SiO_2}}}$, gives—

Silica	53·2
Lead oxide	32·9
Potash	13·9

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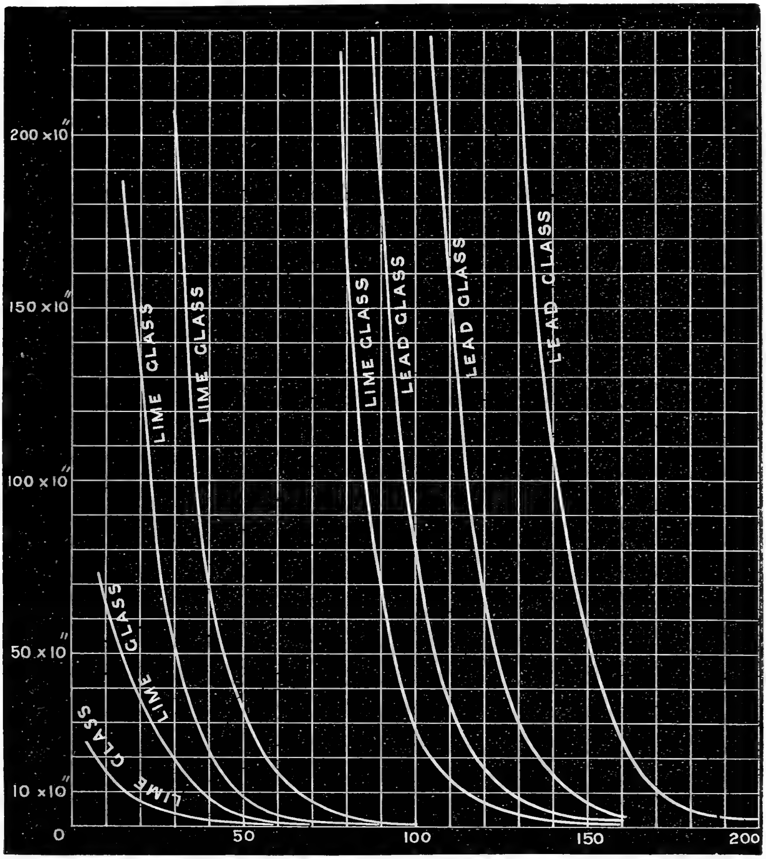
* Ratio of potash to soda may be too high.

FIG. 2.



Allowing the lime, magnesia, &c., to replace one equivalent of lead oxide, this glass very nearly agrees with the above theoretical composition. This therefore ought to be an excellent glass, and so it turns out to be electrically. So far as these results go then, the evidence is in favour of an exact chemical compound for a glass of low conductivity.

In the following table, the resistances at 60° C. of numbers of different specimens of lime glass are given, together with their densities. The first column tells the kind of vessel experimented on; the second the resistance in ohms of a cubic centimetre, and the third the density.

Description of glass vessel.	Resistance in ohms per cub. centim.	Density.
Bohemian tubing	605×10^{11}	2·430
,, beaker	425×10^{11}	2·427
,, ,,	542×10^{11}	2·454
,, ,,	715×10^{11}	2·587
Florence flask	469×10^{9}	2·523
French ,,	996×10^{9}	2·533
Japanese globe	210×10^{10}	2·510
Test-tube	144×10^{9}	2·435
,,	350×10^{9}	2·44
,,	285×10^{10}	2·458
,,	125×10^{9}	2·467
* ,,	147×10^{10}	2·499
* ,,	364×10^{8}	2·53
* ,,	155×10^{9}	2·55
* ,,	374×10^{9}	2·57
,,	196×10^{9}	2·667
* ,,	933×10^{9}	2·547

The specimens marked (*) were of Japanese manufacture; the first four being potash lime glass, and the last soda lime glass. The other test-tubes were supplied from England, and were probably German white glass.

The next table contains a similar comparison for a few specimens of flint glass. The columns have the same meaning as in the last table.

Tumbler of toughened glass	622×10^{10}	2·670
Piece of tubing	389×10^{11}	2·753
Japanese globe	120×10^{12}	2·840
Cylindrical cup with hemispherical base of arsenic-enamel glass	302×10^{12}	3·070
A Thomson's quadrant electrometer jar	102×10^{13}	3·172

“On the Causes of Glacier-Motion.” By WALTER R. BROWNE, M. Inst. C.E., late Fellow Trin. Coll., Cambridge. Communicated by Professor STOKES, Sec. R.S. Received June 1. Read June 15, 1882.

The question of the causes which produce the movement of glaciers, which was at one time so eagerly discussed, would appear to have slumbered for the last ten years. This cannot be said to arise from the fact that a perfectly satisfactory theory has been developed, and recognised as such by all inquirers. The ambiguous allusion to the subject in Sir John Lubbock's presidential address to the British Association is an evidence that such certainty has not been attained. It is, indeed, generally supposed that the fact of the melting-point of ice being lowered by pressure is somehow at the root of the matter; but a full explanation of the origin of this pressure in the case of glaciers, and of the mechanical features of the problem, has yet to be given. I may, therefore, be pardoned if I draw attention to a different solution, proposed not by myself but by one of the greatest of English mechanicians. My apology for doing so is that I approach the question as an engineer, not as a physicist; and that it is in its essence, as will be shown immediately, a mechanical rather than a physical problem.

The following are leading facts of glacier-motion which must be accounted for by any valid theory on the subject:—

(1.) The phenomena of the movement of a glacier are simply those of a solid body in a state of flow.

(2.) The present glaciers of Switzerland or Norway, which are the only ones which have been critically examined, are mere shrunken fragments of the glaciers of the Great Ice Age. To take one instance, the present glacier of the Rhone is about 6 miles long and perhaps 500 feet deep; but the old glacier of the Rhone, which abutted against the Jura, was 120 miles long and must have been 2,000 to 3,000 feet deep. The movement of such glaciers as this must also be accounted for in any satisfactory theory.

(3.) The glaciers of the present day are not confined to the temperate region; they are found in much larger numbers and of much greater size in the Arctic regions.

(4.) Both in the temperate and in the Arctic regions glaciers move in winter as well as in summer, and by night as well as by day.

That a glacier is in a state of flow was first proved by Forbes, and has since been confirmed by the measurements of Tyndall and others. Whilst the whole mass moves downwards, the top moves faster than the bottom and the sides than the middle; the upper layers must therefore be continually shearing over the lower, and the medial over the lateral. A glacier, being a body in a state of flow, must move under the influence of forces powerful enough to overcome its resistance, and so produce this condition.

The general phenomena of the motion of a glacier are exactly reproduced when a viscous body moves through a channel under the influence of its own weight. We have, therefore, to enquire whether the shearing resistance of ice is sufficiently low to enable us to regard a glacier as a viscous mass.

The only experiments known to me on the shearing resistance of ice, are those of Moseley ("Phil. Mag.," January, 1870). He found that with pressures from 100 to 110 lbs. per square inch, cylinders of ice sheared slowly across the two planes in contact, sliding over each other without losing continuity. The distance sheared through was about $\frac{5}{8}$ inch in half an hour. A load of 119 lbs. per square inch was sufficient to shear through a cylinder of $1\frac{1}{2}$ inches in diameter in two to three minutes. From these experiments it would appear that the lowest shearing stress which will cause ice to flow is about 100 lbs. per square inch; but sufficient time was not allowed in the experiments to make this a matter of certainty.

There is another way in which the shearing resistance of ice may be tested. In the case of a block of ice of vertical sides, gravity of course produces a shearing resistance along all planes passing through the base. Let h be the height of such a block in feet, and consider the shearing force due to gravity on any square foot of a plane making an angle θ with the vertical. This shearing force is given by—

$$\frac{\dfrac{wh \times h \tan\theta}{2} \times \cos\theta}{h \sec\theta} = \frac{wh}{2} \sin\theta \cos\theta.$$

This expression is a maximum when $\theta = 45°$, and its value is then—

$$\frac{wh}{4}.$$

What is the greatest height at which a vertical cliff of ice will stand? I am not able to state this precisely, but it is very considerable. Mr. Whymper mentions crevasses in South America 300 feet deep. Cliffs of fully that height have been seen standing out of water in the case of icebergs, and as so small a part of an iceberg projects above water, these cliffs probably extend below to a considerable depth. Taking, however, only 300 feet for the value of h, or for the maximum height of an ice cliff, this would give about 30 lbs. per square inch as the lowest shearing force upon a plane of ice which would cause it to assume the condition of flow.

Let us now suppose a glacier of thickness a, lying upon a slope whose inclination to the horizontal is β: then the force per square foot, tending to shear the ice at its junction with the slope, is clearly $aw \sin\beta$.

Supposing $\sin\beta$ to equal $\frac{1}{4}$, and that the shearing resistance is 30 lbs. per square inch, we get $a =$ about 290. Hence we may say that a glacier lying on a slope of 1 in 4 will not move at all under its own weight, unless it be at least 300 feet thick, and that if it be more than this, the upper 300 feet will move as one solid mass, the part below alone representing the conditions of flow.

It is needless to say that there are hundreds of glaciers which are less than 300 feet thick, and which at no part of their course have a slope anything approaching 1 in 4.

We have now to show that the theories generally propounded for glacier action are all of them negatived by some of the foregoing considerations. These theories may be stated as follows:—

(1.) The glacier simply slides over its bed as a solid body. This is negatived by the fact that some parts move faster than others.

(2.) The glacier flows under the action of its own weight, exactly as a viscous body flows. This is the theory of Forbes. It is disproved by the facts given above, which show that even on a slope of 1 in 4 a glacier would not flow unless it was at least 300 feet thick.

(3.) The glacier moves by the crushing of its base. This has been disproved by Moseley's experiments, which showed that the crushing resistance of ice was considerably higher than the shearing resistance.

(4.) The glacier moves by the melting of its base. This is the theory of Hopkins. He placed a block of ice at 32° F. on a slab at a small angle, and found that it slowly descended as it melted. On this view the bottom of the glacier must always be in a melting state. But glaciers are of all sizes and thicknesses, and they move in winter as well as summer. Bessels ("Die Amerikanische Nordpol Expedition," p. 398) measured the motion of an Arctic glacier (not apparently very thick), in the month of April, which is just when the winter cold would have sunk deepest, and found it considerable. Again in the "Zeitschrift des deutschen Geologischen Gesellschaft," vol. 33, p. 693, is an account of measurements of a Greenland glacier, both in winter and summer, which show that the motion in winter is only 20 per cent. less than in summer. It has been suggested to me that the interior heat of the earth may be sufficient to keep the bottom of the ice from freezing; but this cannot apply near the sides, where the ice is shallow, and the freezing of a very small strip on each side would be sufficient to keep the whole mass from descending. Moreover, this cause would apply to masses of snow as much as to ice. But it is known that masses of snow, though lying on steep slopes, do not descend in this way, even in summer, but melt away where they lie.

(5.) According to the theories of Tyndall, Croll, and others, the glacier moves not in the form of ice, but of water. These theories are based on the known fact that the freezing point of ice is lowered by pressure. Hence it is supposed that certain parts of a glacier are continually being exposed to so much pressure that they melt. The water escapes downward, and the pressure being relieved, it freezes again. The continuity of the glacier is further kept up by the process of regelation, according to which two pieces of ice if placed in contact, form into one solid mass.

The advocates of this theory hardly seem to consider how very small the lowering of the freezing point is for any ordinary pressure.

It is only ·0075° per atmosphere. In other words, it will require a pressure of 2,000 lbs. per square inch to liquefy ice at 31° instead of 32°. This is equivalent to the weight of a column of ice about 5,000 feet high. It is needless to ask whether such a pressure can exist within an ordinary glacier, while on the other hand glaciers undoubtedly move at temperatures far below freezing point—in the Arctic regions below zero.

It seems to be generally supposed that the pressure in the lower part of a glacier is due to the steeper upper portions: the glacier channel is spoken of as a mould, through which the ice is forced by pressure from behind. But in the upper glacier, slopes of ice or *nevé* are not uncommon at angles of 30°, or even more. Such slopes usually do not even touch the more level parts of the glacier below them, but are separated from them by a wide, deep crevasse called a Bergschrund. Of this the well-known ice wall of the Strahleck is a conspicuous example. In other cases such slopes do not end in a glacier at all, but die away upon the mountain side. It is certain, therefore, that ice or *nevé* is able to maintain itself at a high angle upon its slope of rocks, and therefore cannot possibly exercise pressure upon the parts of the glacier far in advance of its foot. The fallacy of this idea may be further illustrated by referring, not to modern glaciers, but to those of the Great Ice Age. Can we suppose that the pressure of the snows about the sources of the Rhone was sufficient to drive that glacier down the valley to Martigny, round a sharp angle to the Lake of Geneva, through the bed of that lake, and on to the slopes of the Jura, a distance of more than 100 miles, in which the average slope was about 1 in 200; giving a propelling force per ton of ice of about 11 lbs. only?

All these theories have this in common, that they regard gravity as the sole and direct agent in the movement of glaciers, and the above considerations seem to prove that it is an agent far too weak for the work it has to do.*

The only other agent which has been suggested, or seems likely to be suggested, to account for the motion of glaciers, is heat. This suggestion, as is well known, is due to the late Canon Moseley, F.R.S., and was to some extent worked out by him in papers published in the "Phil. Mag." 1869 and 1870.

The mode of operation, on this theory, is well known. Ice is here considered merely as a solid body, obeying the ordinary laws of expansion and contraction under differences of temperature. This it is known to do, the coefficient of linear expansion, for 1° F., being

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* Another evidence against pressure from behind as a cause of motion is furnished by the very small size of many glaciers. Some of these, notably those of the class called "glaciers remaniés," are only a few hundred yards long, and cannot be many feet deep.

·00002856 (Moseley, "Phil. Mag.," January, 1870), which is very high. When a mass of ice, such as a glacier, suffers a rise in temperature, either through conduction or radiation, it will expand; this expansion will take place mainly in the direction where movement is easiest, that is, down the valley. If from any cause the temperature falls, the glacier will again contract; but since the expansion is assisted by gravity whilst the contraction is opposed by it, the latter will be somewhat less in amount than the former, and when the ice has returned to its original temperature its centre of gravity will have moved a certain small distance down the valley. By such alternate expansions and contractions the glacier moves gradually from the top to the bottom of its course.

That variations of temperature do take place in a glacier cannot be doubted, whatever be the condition in which it lies. This granted, the fact that it should move in the way described appears to me no more surprising than that the sheets of lead on which Canon Moseley made his well-known experiments did so move; and that the motion thus produced is of the character which answers to all the facts of the case, so far as they are at present known, can, I believe, be established.

The controversy occasioned by Canon Moseley's articles was unfortunately terminated by his illness and death, before the matter had been fully cleared up. The main objections urged to his theory were two. The first was that a glacier is not one continuous body (as assumed by Canon Moseley in his mathematical investigation), but is broken up into many parts by crevasses. But in the first place, the assumption above mentioned is merely one of convenience, and not in the least necessary to the theory. A detached piece of ice would move in the same way as a glacier, or as the sheet of lead did in Canon Moseley's experiments. Secondly, if a glacier is anywhere divided in its whole thickness by a crevasse, this is absolutely fatal to the gravitation theories, since there can be no pressure between the portions above and below this division. The only possible explanation of crevasses, on these theories, is that they are due to the glacier bending over a convex part of its bed. In that case the bottom half will be in compression, and only the top half in tension, so that the crevasse cannot possibly extend more than half way through the thickness.

The second objection was that the conductivity of ice is low; hence the effect of the heat would be confined to the layers near the surface, and could not account for the motion of the glacier as a whole. This objection does not seem to be confirmed by careful reflection upon the way in which such forces act. Let us suppose a glacier 100 feet deep, of which each successive foot expands and contracts alike throughout, but adheres with a definite shearing resistance to the layers above and below. Let there be a rise in temperature, which does not extend beyond the uppermost 10 feet. This layer will expand, and if it were free would expand to the full amount due to the increase in temperature. But its lower surface is not free. In expanding it will therefore drag the next layer after it, or in other words will cause it to expand also. The amount of expansion, however, will not be so great, because there will be a certain shearing extension at the plane of division between the two. The second layer will similarly cause an expansion in the third, and so on to the bottom. In consequence, the energy which would all have been exerted on the top layer, had that been free, will be distributed over the whole of the layers; and the extension of the top layers will of course be much smaller than it otherwise would have been. Should the temperature then remain constant, the layers will retain their position, and adapt themselves to the new circumstances. If the temperature falls the layers will contract; but from the now opposing effect of gravity they will not return to their original position. The top layer which has extended furthest will be the furthest below its original position; the second layer next, and so on. If we suppose the layers to be indefinitely thin, we have the condition of things in an actual glacier. The ice in any vertical section will, on the whole, move down the slope, but the top will move faster than the middle, and the middle than the bottom, exactly as it is known to do. The same holds with regard to a horizontal section. At the sides the ice will be held back, not only by the friction, but also by the protuberances of the rock, which compel the ice to shear over them. Hence the velocity there will be retarded, and will be less than that in the middle, which is comparatively free.

A more important objection remains to be considered, which is this. On the present theory the motion at any point on the surface of a glacier will be not continuous, but oscillating alternately downwards and upwards, and the nett distance by which it has descended, say, in a day, will be a mere fraction of the total distance through which it has moved in that period. If so, this alternate motion ought to have been noticed in the various observations which have been made upon glaciers, and this does not appear to have been the case. But, in reply to this, it may be remarked that most of the observations have only given the nett movement of points on the glacier during intervals of a day or more, and therefore would not show the oscillations. Again, such observations have always been at points near the end of a glacier. Now the variations in temperature of a glacier will be very different at different parts, and the motion of the end of the glacier will, to a great extent, show the average result of these different advances and retreats in different parts of the higher regions. This average result will, of course, be a steady progression down the valley, and the oscillatory movement at the end of the glacier may be so much masked by this as not to be readily observable. Lastly, it may be suggested as possible that a certain amount of expansion by heat may have the effect of giving a *set* to ice, so that it does not return to its original length when brought back to the same temperature. If this be so the oscillations would be much less marked, and at the end of the glacier would probably be indistinguishable.

I may now draw attention to some phenomena of glacier action, which are explained by the heat theory, but which do not seem explicable on the gravitation theory.

(1.) It is well known that glaciers, when they emerge from a narrow gorge into a comparatively wide valley, spread out into a fan-shape. The Rhone glacier is a well-known instance. A still better one is a small glacier in Norway, mentioned by Professor Sexa, which spreads out to five or six times its previous width. Now the effect of gravity, acting on a mass as a whole, is to carry it in one single direction, that of the steepest slope. The only way in which gravity can produce such a spreading out is by the parts of the glacier shearing over each other in the manner of a viscous solid. But the phenomena of ice cliffs, as mentioned above, show that ice does not spread from this cause, so that the fact seems impossible to explain by gravitation alone. On the heat theory it is, of course, perfectly easy: the expansion and contraction will take place in all directions where there is freedom to move.

(2.) Connected with this phenomenon is that of the longitudinal crevasses seen near the edges of glaciers, and particularly where they spread out in the manner just described. Now on the gravitation theory, as remarked above, the only possible explanation of a crevasse is that the ice is bending over a convex surface, and that its upper part is thus placed in a state of tension, under which it breaks. Since, on the gravitation theory, every part of a glacier is exposed to a severe pressure from behind, this explanation does not fit very well even for transverse crevasses; but to longitudinal crevasses it is clearly inapplicable, since the bottom of a valley is seldom or never convex in the direction of its width. On the heat theory the explanation is simple. We may suppose the heat energy communicated per square foot of surface to be about the same, whether near the middle or edge of a glacier. This energy is expended in producing an expansion throughout the whole thickness of the glacier, as described above. Hence the smaller this thickness, the greater will be the amount of expansion, and the greater therefore the nett motion which results. Hence the thinner parts of a glacier will always be tending to tear themselves off from the thicker, and thus longitudinal crevasses will frequently be found.

(3.) The striæ which are so marked a feature of glacier-worn rocks become more easily explained on this theory. I have seen such striæ, even in the hard hypersthene of Skye, which were a considerable fraction of an inch in depth. When we consider the enormous force necessary to plough out such a furrow in hard rock, it is almost impossible to believe that it was done by the simple passage over it, once for all, of a stone imbedded in the ice. If, however, the stone descended by a series of oscillations, so that it passed many times over the same spot, this difficulty is greatly lessened.

(4.) In conclusion I may point out that the advocates of the gravitation theory are bound to explain what becomes of the heat energy which is poured into a glacier. When the sun is shining this radiant energy is always very large, although the temperature of the air may be low. In such cases the glacier does not melt; it is perfectly clear that it must expand, as any other solid must expand under the action of heat. If so, it seems unreasonable not to hold that the gradual descent by alternate expansion and contraction must follow, as it is known to follow in the case of other materials.

On the subject of the motion of Arctic ice, Dr. Rae, F.R.S., has kindly permitted the publication of the following particulars:—

"When in Greenland, in the autumn of 1866, I was ice-bound at the head of one of the fiords, and slept a couple of nights at an Eskimo's house. A glacier about half a mile distant was then in full activity, the movement of which might, I believe, have been as visible to the eye as it certainly was audible to the ear.

"My own idea is that Arctic glaciers must have a downward motion more or less during the whole year, summer and winter. I believe the alternations of heat and cold—or, I should rather say, of temperature—would of itself cause motion, especially near the upper surface.

"We know that ice 2 or 3 feet or more thick, contracts very considerably in a few hours by a sudden fall of fifteen or twenty degrees of temperature. I have found cracks in Lake Winnipeg 3 or 4 feet wide, formed by this cause during a single night, almost stopping our sledge journey. This gap soon freezes up. Then the weather gets milder, the ice expands, and with the new additional formation is too large for the lake, and is forced up into ridges. This process goes on at every 'cold snap,'* alternating with milder weather. Now supposing a glacier for 10 or more feet of its depth contracts by cold, as lake ice is known to do, it will get a series of cracks probably in its longest axis, say from inland seaward; the first snow-drift will fill up these cracks or some of them, and this filling up will to some extent perform the same office as the freezing of the cracks in the lakes. The longitudinal extent of the glacier will be increased. A snow-storm always brings milder weather, which would expand the glacier, but as this expansion would naturally tend downhill, instead of up, the whole motion would be downwards. But even if the cracks I mention did not take place, the contraction by cold would pull the ice downhill, not up, whilst the expansion by increase of temperature would tend to *push* the glacier downhill, so that these opposite actions would produce similar effects in moving the glacier, or such part of it as could be acted upon by external temperature, downwards.

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* "Cold snap," an American term meaning a rather sudden increase of cold.

"I may also add that when a crack, however slight, is formed by contraction, the cold is admitted into the body of the glacier, and increases the contracting power or influences."

"On Impact with a Liquid Surface." By A. M. WORTHINGTON, M.A. Communicated by Professor OSBORNE REYNOLDS, F.R.S. Received January 27. Read February 16, 1882.

[This paper is made up of the abstract already printed in the "Proceedings"—here reprinted for the convenience of the reader—and a selection of the figures sent in by the author, together with a description of them extracted from the original paper.]

The apparatus previously used* by the author for following the progress of the splash of liquid drops impinging on a solid plate has been improved. The main principle of the method by which successive stages are isolated and rendered visible remains the same, viz., instantaneous illumination at any desired stage by means of the primary spark of an induction coil; but the timing of the illumination is now effected by a timing-sphere let fall simultaneously with the solid or liquid sphere whose impact is to be observed. The timing-sphere strikes a plate whose height can be adjusted, and thereby starts the mechanical action which results in the spark.

The time interval between successive stages of the disturbance can be measured to within a few thousandths of a second.

The significant portion of the whole series of changes in most of the splashes observed is comprised within about one-third of a second. The impact of both solid and liquid spheres has been studied, and is illustrated by several series of drawings which accompany the paper.

Milk drops falling into water were found to produce a similar disturbance to that resulting from the impact of similar water drops, and were used for the sake of distinguishing the original liquid of the drop from that into which it fell. With a drop about 5 millims. in diameter, falling from less than 1 metre, an annular rim is raised at the first moment of impact, bounding a hollow which is afterwards characterised by regularly disposed radial ribs and arms, at the bottom of which the drop descends, passing below the surface and becoming completely submerged to emerge again at the head of a column of adherent liquid, but with its upper portion apparently unwetted by the liquid with which it has been covered. The column then subsides, and the liquid of the original drop is seen to pass into the well-known vortex ring which descends through the liquid.

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* "Proc. Roy. Soc.," vol. 25, pp. 261, 498.

The influence of velocity of impact in modifying the phenomenon is shown by the drawings.

When the drop is large, and the fall considerable, the rim thrown up takes the form of a hollow crater-like shell of liquid, the mouth of which closes over the drop, imprisoning air which may remain as a bubble on the surface. This is the bubble seen when large rain drops fall into water. Observations of the bursting of this bubble confirm incidentally the explanation lately given by J. Plateau of the manner of bursting of a soap bubble.

The splash of a milk drop in petroleum and in olive oil is also described. The course of phenomena is very similar to that in water, modified however by the greater or less mobility of the liquids in question.

The impact of solid spheres is then described. The nature of the disturbance produced, with a given velocity of impact, is found to depend entirely on the state of the surface of the sphere.

A polished and perfectly dry sphere of ivory or marble 1 to 3 centims. in diameter, let fall from a height not exceeding 1 metre, is apparently wetted at once, and is seen to be sheathed with liquid before the whole is below the average level of the surface. The disturbance of the surface is very slight.

The same sphere if *rough* or *wet* with the liquid in question, behaves quite differently, making a very deep impression, similar at first to that produced by a liquid drop, which finally becomes an almost cylindrical column of air within the liquid, part of which afterwards rises as bubbles while a portion descends in the wake of the sphere.

The influence of roughness in hindering the spread of liquid over the surface of the impinging sphere is then pointed out.

At the close of the paper an explanation is put forward of the radial ribs, arms, and striæ which are a notable feature of all splashes. Measurements of the annular rim bordering a thin central film into which a drop falling upon a plate passes,* show that the number of the lobes and arms which are subsequently observed, agrees well with the number of drops into which such an annulus would theoretically tend to split if unhindered by friction with the plate on which it rests, and it is then pointed out that the effect of the connecting film would be exactly such as to counteract the influence of this friction.

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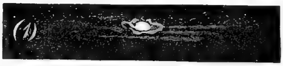
* "Proc. Roy. Soc.," vol. 25, p. 500, fig. 4.

In the same way the radial striæ and ribs which characterise the hollow formed round a drop or solid sphere impinging on a liquid surface, are accounted for by the instability of the annular rim of the hollow, which through its tendency to cleave into a definite number of drops, determines a corresponding number of lines of easiest flow, at each of which a rib or arm is developed.

The author has observed that after the details have been once revealed by the method of instantaneous illumination, it is not difficult to identify the broad features of any splash that may occur by attentive observation in continuous light. Such observation may afford valuable information as to the condition of the surface of an impinging solid.

[What follows is extracted from the "Description of the Phenomena."]

SERIES I.



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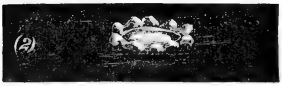
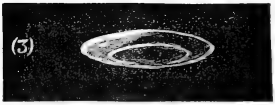
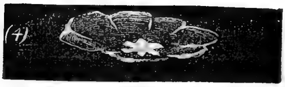


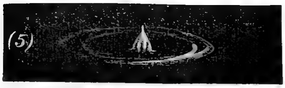
$\tau = 0.$



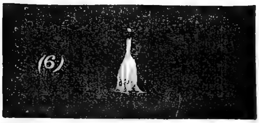
$\tau = \cdot 0097.$

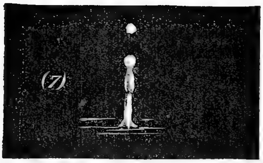


$\tau = \cdot 0392.$

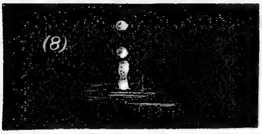


$\tau = \cdot 0392.$

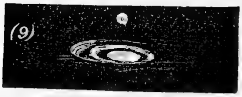




$\tau = \cdot 0979.$



$\tau = \cdot 1095.$



$\tau = \cdot 167.$

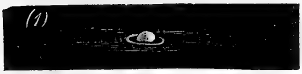
Series I shows the splash produced by a milk-drop ·496 centim. in diameter falling into water from a height of 8 centims., which differed in no material respect from that due to a fall of 6 centims., which was the lowest that I could observe.

[In most of the figures of these and the succeeding series the disturbing drop or sphere is in the centre; the white parts round it represent those raised portions of the liquid which catch the light. The numbers at the side of each figure give the time interval in seconds from the occurrence of the first figure.]

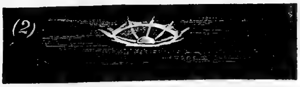
In respect of the agitation communicated to the liquid the phenomenon already differs from that which occurs with a fall of less than 1 centim. A hollow is formed with a raised lobed edge, into which the drop descends, at first flattened out against the bottom (fig. 2). The hollow as the drop descends becomes wider and deeper, but finally (fig. 3) closes over the drop, which, however, soon again emerges as the hollow flattens out, appearing first near, but still below, the surface (in fig. 4) in a flattened lobed form, and afterwards rising as a column, somewhat mixed with adherent water, in which traces of the lobes are at first very visible. The origin of these lobes is explained at the close of the paper.

The column which is elevated being nearly cylindrical is approximately subject to the same laws of stability as a cylinder, and accordingly divides into drops before or during its descent again into the liquid. As it disappears below the surface, the outward and downward flow causes a hollow to be again formed, up the sides of which an annulus of milk is carried, while the remainder descends, to be torn again a second time into a vortex ring, which however, is always liable to disturbance from the falling in of drops which once formed the upper part of the rebounding column. Thus it generally happens that, owing to these subordinate drops, no distinct vortex ring is formed. Sometimes, on the other hand, two are formed in quick succession. It is, however, in the next series that the formation of the vortex ring is most clearly traced, and it is on the evidence there afforded that the interpretation just given is really based.

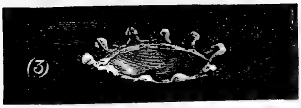
SERIES II.



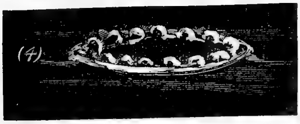
$\tau=0.$



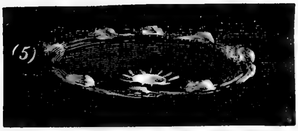
$\tau=\cdot 00314.$



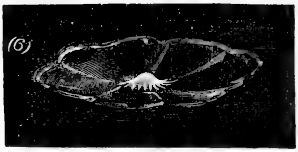
$\tau=\cdot 0137.$



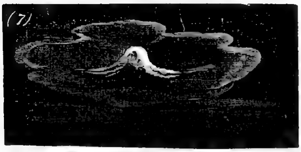
$\tau=\cdot 0389.$



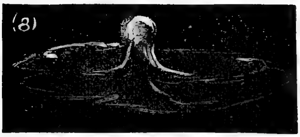
$\tau=\cdot 0498.$



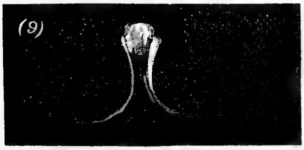
$\tau=\cdot 0551.$

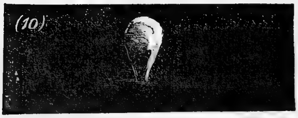


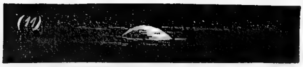
$\tau=\cdot 0759.$

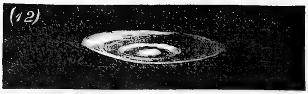


$\tau=\cdot 0901.$

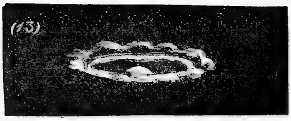


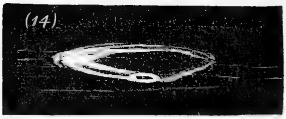






$\tau = \cdot 295$.





Series II exhibits the splash of a drop of milk ·502 centim. in diameter, falling from a height of 43 centims. into water. All the characteristics of the last splash are here more strongly marked. The hollow is deeper and wider, the drop descending farther below the surface before it is completely covered. Rays, whose origin will be explained hereafter, are shot out symmetrically from the centre. Their number seemed to vary a good deal, and I have made no attempt to select drawings which are in agreement in this respect. It will be understood that these rays contain little or none of the liquid of the drop, which remains collected together at the centre.

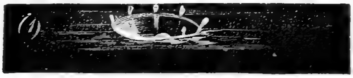
The drop, after being completely covered and lost to sight (figs. 3 and 4), emerges as before (figs. 5 and 6), but the energy of the impact, instead of being expended in raising the same amount of liquid to a greater height, is now spent in lifting a much thicker adherent column to about the same height.

There was sometimes noticed, as is seen in fig. 9, a tendency in the water to flow up past the milk, which, still comparatively unmixed with water, forms the top of the emergent column.

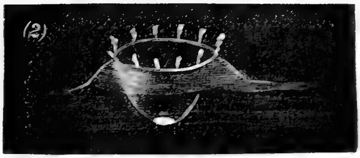
The greater relative thickness of this column prevents it splitting, and figs. 10 and 11 show it descending below the surface to form the hollow of fig. 12, up the sides of which an annular film of milk is carried (figs. 12 and 13), having been detached from the central mass, which descends to be torn again, this time centrally, into a vortex ring.

On increasing the height of fall to 1 metre and retaining the same size of drop, no great change in the nature of the splash takes place, but the emergent column is rather higher and thinner, and shows a tendency to split into drops. It does not, however, succeed in doing so, and a well-defined vortex is produced as before. But on letting fall into water a drop of twice the volume (·625 centim. in diameter) from the height of 133 centims. the splash of Series III is obtained, which is beginning to assume quite a different character.

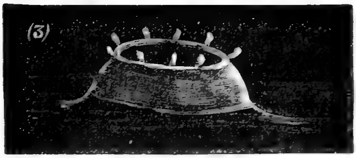
SERIES III.



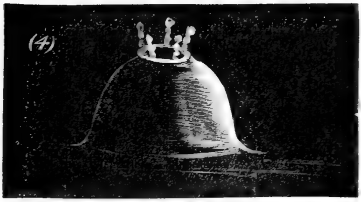
$\tau = 0.$



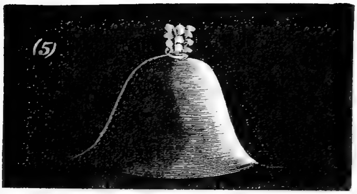
$\tau = 0021.$



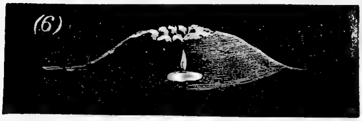
$\tau = \cdot 0042.$



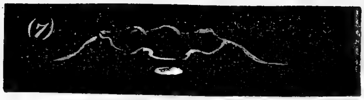
$\tau = \cdot 0165.$



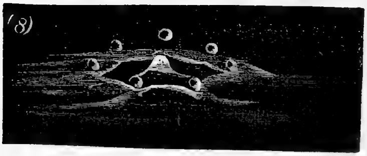
$\tau = \cdot 0206.$



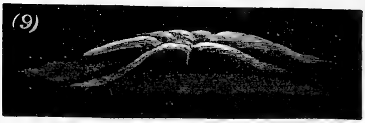
(6) $\tau = \cdot 0443$.



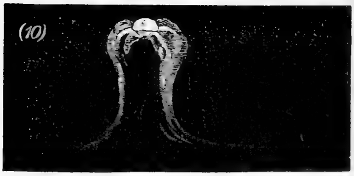
(7) $\tau = \cdot 0482$.



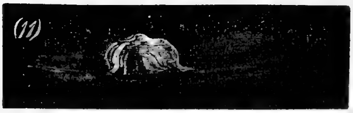
(8) $\tau = \cdot 0595$.



(9) $\tau = \cdot 0707$.



(10)



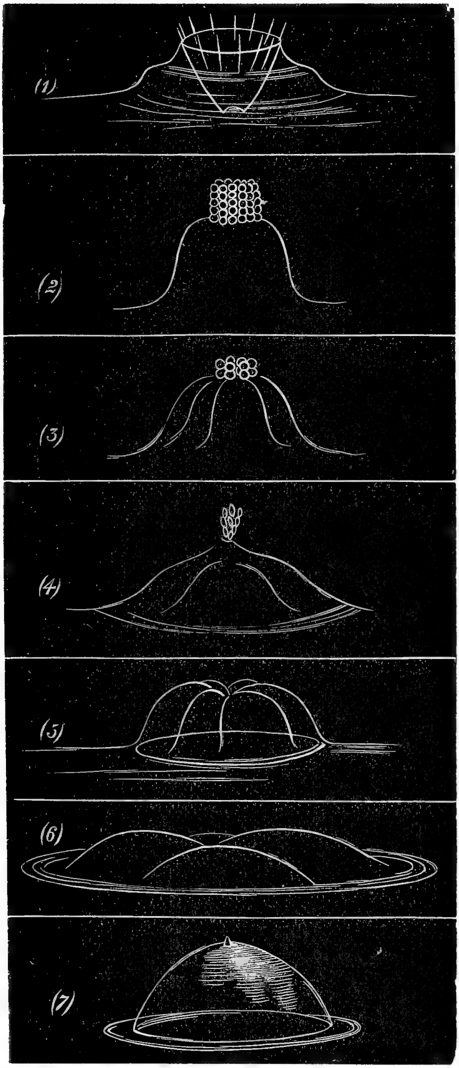
(11)

The raised rim of the previous series is now developed into a hollow shell of considerable height, which tends to close over the drop.

This shell is a characteristic feature of all splashes made by large drops falling from a considerable height and is extremely beautiful. In the splash at present under consideration it does not always succeed in closing, but opens out as it subsides and is followed by the emergence of the drop which is now accompanied by so considerable a quantity of adherent liquid that the vortex ring which is formed is not generally well defined. At other times the shell closed completely, but soon opened again and the configurations (7) to (11) followed.

At other times, again, the shell having once closed over the imprisoned air never opened again; the liquid composing it flowed down

Series IV.



the sides, which thus became thinner and thinner till at last a large bubble remained floating on the top of the water. Series IV shows the formation of such a bubble.

It will be observed that the flow of liquid down the sides is chiefly along definite channels, which are probably determined by the arms thrown out at an earlier stage. The bubble is generally creased by the weight of the liquid in these channels.

It must be remembered that the base of the bubble is in a state of oscillation, and that it is liable to burst at any moment, when such figures as (6) and (7) of Series III, &c., will occur. These figures are in complete accord with the explanation lately given by M. J. Plateau* of the phenomena presented by a bursting bubble.

Such bubbles as those described are often seen when large rain-drops fall into smooth water. No vortex ring is produced, and the disturbance caused by such a drop is confined to a very small depth, the liquid flowing off laterally in all directions down the comparatively gentle slope of the sides of the shell.

Series V shows the splash of a drop of milk ·496 centim. in diameter falling into olive oil from a height of 60 centims. The last four figures are interesting, since they prove the lateral as well as downward flow of the oil, which as it flows away from the vortex drags with it the adherent milk, and, as it were, turns the drop inside out till it recovers itself by irregular jerks under the influence of a surface tension, which at length asserts itself. This jerky recovery may be perceived by attentive observation in continuous light without the aid of any apparatus.

Impact of Solid Spheres.

It has been already mentioned incidentally that an increase of viscosity in the impinging drop, produced by the addition of glycerine, has its effect on the character of the splash, and it now remains to describe the splash produced by the impact of a solid sphere.

To my great surprise I found that the character of the splash, at any rate up to a height of fall of 150 centims., depends entirely on the state of the surface of the sphere.

A polished sphere of marble, 1·28† centims. in diameter, rubbed very dry with a cloth just beforehand, and dropped from a height of 62 centims. into water, gave the figures of Series VI, in which it is seen that the water spreads over the sphere so rapidly that it is sheathed with the liquid even before it is below the general level of

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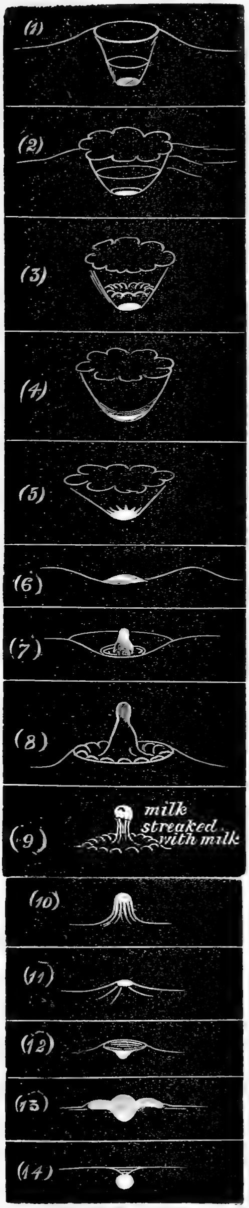
* "Quelques Expériences sur les Lames Liquides Minces." "Bulletins de l'Académie Royale de Belgique," 3me série, tome II, No. 7, 1881.

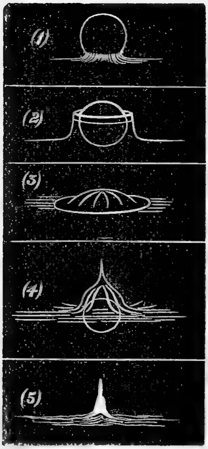
† This or a similar sphere 1·55 centims. in diameter could be used indifferently.

SERIES V.


(1)
(2)
(3)
(4)
(5)
(6)
(7)
(8)
(9)
milk
streaked
with milk
(10)
(11)
(12)
(13)
(14)

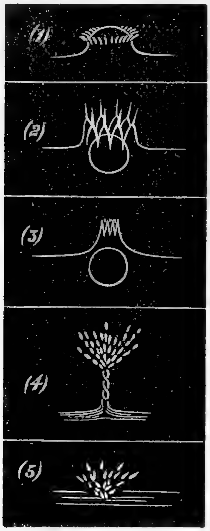
the surface. The splash is insignificantly small and of very short duration.

Series VI.



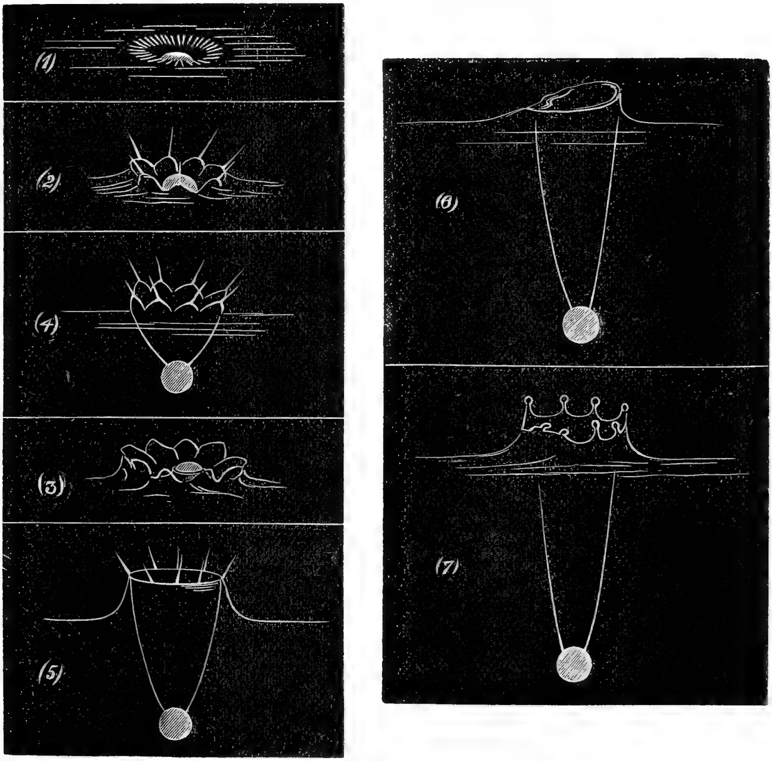
If the drying and polishing be not so perfect, Series VII is produced; while if the sphere be roughened with sand-paper or *left wet*,

Series VII.



Series VIII is obtained, in which it will be perceived that the sphere is followed by a cone of apparently adherent air, while the water seems to find great difficulty in wetting the surface completely.

Series VIII.

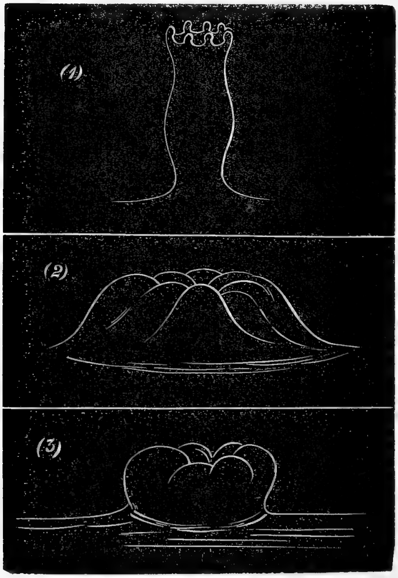


Part of this column of air was carried down to a depth of at least 40 centims., and then detached only when the sphere struck the bottom of the vessel.

The last two figures show the side of the crater falling in, but this did not always happen; the walls often closed over the hollow, exactly as in Series III, figs. 4 and 5. Whether the air column meanwhile splits into bubbles in other places below the surface, I could not observe, but it is very probable that it does so, since its form is too nearly cylindrical for its equilibrium to be stable. When the fall was increased to 151 centims., the figures of Series IX were obtained.

The tube of fig. 1 corresponds to the shell of Series III and IV, and is not only elevated to a surprising height but is also in the act of cleaving, the outline being approximately that of the unduloid of M. Plateau.* Figs. (2) and (3) show the bubble formed by the closing of this tube weighed down in the centre by the liquid at the lip of fig. 1, and correspond to figs. (5) and (6) of Series IV.

SERIES IX.



With other liquids, such as petroleum and alcohol, similar results are obtained.

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* M. J. Plateau has recently studied the formation of bubbles by the cleavage of such a cylindrical film. See note, p. 226.

"Sun-spots and Terrestrial Phenomena. I. On the Variations of the Daily Range of Atmospheric Temperature, as recorded at the Colaba Observatory, Bombay." By CHARLES CHAMBERS, F.R.S., Superintendent. Received May 30. Read June 15, 1882.

The present investigation is on the model of one by Dr. Balfour Stewart, published in the "Proc. Roy. Soc." (vol. 25, p. 577), and dealing with similar records obtained at the Kew Observatory. The records extend, in the present case, from June 1, 1847, to December 31, 1880, and consist of excesses of the highest temperature observed above the lowest temperature observed on every observation day of the period, and all are therefore necessarily positive numbers. Until the end of the year 1851, the observation day was the Göttingen Astronomical day: since that time it has been the Bombay Civil day. The daily excesses were obtained for the period from 1847·42 to 1873·0, from hourly observations made on all days except Sundays and a few holidays, and for the period from 1873·0 to 1881·0 from daily readings of self-registering maximum and minimum thermometers taken on all days without exception; for all days previous to 1866·0 they are differences of uncorrected readings of the dry-bulb thermometer, but for all later days every individual thermometer reading was corrected for the error of graduation of the thermometer before the differences were taken. The maximum thermometer is a mercurial one of the construction known as Negretti and Zambra's: the minimum is a spirit thermometer, of which the chamber at the top of the tube is bent upward, so as to prevent the accumulation in it of condensed spirit; the instrument was tested in 1865, and again in 1881, and it was found on the latter occasion to have practically the same error as before. The dry bulbs (up to 1873·0) were mercurial thermometers by Newman, with scales divided to fifths of a degree.

A. *Annual Variation of Temperature-Range.*

B. *Variation of Long Period.*

2. Table I exhibits the monthly and annual means of the daily temperature-range.

Changes were made in the mode of exposure of the thermometers, of a character likely to affect the comparability of the preceding and following observations, in 1851 and again on 1866·0; but the annual means in the following table seem to imply that the state of things holding in 1851 was maintained for some time longer, and as the only event on record that would suggest a definite limit to that time is a change of thermometers on the 31st March, 1852, we have divided the

Table I.—Containing Monthly and Annual Means of Daily Temperature-Range expressed in Fahrenheit degrees.

Year.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Mean of year.
1847	...	...	...	...	...	8·1	7·3	8·2	8·7	12·2	15·4	16·2	
1848	17·6	15·4	13·8	11·4	10·7	8·2	7·4	6·8	9·5	11·0	15·4	15·3	11·9
1849	14·9	13·9	13·2	11·9	9·9	8·2	7·2	6·7	7·2	12·3	15·0	16·5	11·4
1850	14·9	18·2	13·1	11·7	10·3	9·9	8·2	8·1	9·0	11·9	15·9	15·3	12·2
1851	15·8	15·2	13·4	12·7	10·6	9·1	6·7	7·5	9·4	11·3	13·0	16·2	11·7
1852	16·0	14·8	14·1	13·2	11·8	9·3	7·5	7·2	8·0	11·1	15·2	12·9	11·8
1853	15·8	15·0	14·7	12·6	11·9	8·5	6·5	8·5	8·3	13·1	14·3	16·1	12·1
1854	15·4	15·9	14·4	12·4	11·5	9·2	6·5	7·9	8·3	11·5	13·1	12·1	11·5
1855	13·8	12·6	12·8	12·2	11·5	9·0	7·6	9·0	9·2	11·2	13·9	14·1	11·4
1856	13·8	13·6	11·8	12·2	11·2	8·8	6·6	7·8	8·4	10·4	13·3	13·5	10·9
1857	12·6	14·8	14·1	12·2	10·8	9·7	7·4	5·4	7·3	11·2	13·2	14·9	11·1
1858	15·0	14·2	11·8	12·1	10·6	9·4	7·6	7·2	8·3	11·4	13·5	13·1	11·2
1859	13·8	12·9	13·5	11·9	10·3	9·0	8·0	6·8	8·3	10·2	12·7	14·0	10·9
1860	14·8	12·1	12·0	10·1	9·6	9·1	5·9	7·4	7·7	10·0	15·4	15·2	10·8
1861	14·3	15·3	12·5	11·3	10·7	8·8	5·6	5·4	8·1	9·4	13·6	14·1	10·8
1862	13·6	14·7	13·3	11·5	10·5	8·3	7·4	7·7	7·8	10·2	12·8	15·2	11·1
1863	14·6	16·5	11·8	11·5	10·6	7·4	7·0	6·7	8·4	11·0	14·0	13·9	11·1
1864	15·1	14·0	13·0	10·9	10·4	8·9	6·4	7·0	9·2	12·1	12·6	14·7	11·2
1865	14·6	13·3	12·6	11·3	10·2	9·5	7·0	6·3	9·2	11·1	13·6	12·5	10·9
1866	13·4	13·7	10·8	10·9	9·2	7·0	5·9	5·6	7·9	10·6	14·0	13·3	10·2
1867	13·0	13·7	11·6	9·5	8·7	7·3	6·0	5·6	6·8	10·0	12·3	13·2	9·8
1868	13·0	12·8	11·0	9·9	9·1	7·3	6·4	5·9	7·7	10·9	13·6	13·4	10·1
1869	13·0	12·4	10·6	10·0	9·2	7·3	6·3	6·1	6·6	9·5	12·1	10·6	9·5
1870	12·5	12·9	10·9	10·5	8·7	7·5	5·3	6·3	7·0	8·9	10·8	12·0	9·4
1871	12·8	12·4	11·7	10·2	8·6	7·5	6·5	5·9	6·9	10·4	11·1	11·9	9·7
1872	13·5	14·2	11·1	10·4	9·0	7·3	5·3	6·2	7·1	10·4	13·4	11·1	9·9
1873	13·6	11·4	10·3	9·4	8·6	6·5	4·8	5·8	7·0	11·2	12·3	13·8	9·6
1874	13·5	13·3	11·2	10·0	8·4	7·1	5·4	6·3	6·5	10·4	12·1	12·5	9·7
1875	13·4	11·6	11·0	10·0	8·9	7·2	6·7	6·4	6·1	10·1	12·4	12·6	9·7
1876	13·0	13·1	10·6	9·8	9·0	7·5	5·9	6·5	8·0	11·3	12·3	13·8	10·1
1877	12·3	12·0	11·5	10·6	9·3	8·1	7·4	7·2	8·4	8·8	12·3	10·9	9·9
1878	12·8	13·5	11·3	10·7	9·4	8·4	6·8	6·5	7·7	10·4	10·6	14·1	10·2
1879	14·1	12·6	12·6	10·6	9·0	7·2	7·0	6·0	7·8	10·4	13·8	15·3	10·5
1880	14·6	13·6	11·2	10·4	9·3	8·2	6·7	7·2	7·3	10·1	12·9	13·9	10·4
Means for groups of years.													
1848 to 1851	15·8	15·7	13·4	11·9	10·4	8·8	7·4	7·3	8·8	11·6	14·8	15·8	11·8
1853 to 1865	14·4	14·2	12·9	11·7	10·8	8·9	6·9	7·2	8·3	11·0	13·5	14·1	11·2
1866 to 1872	13·0	13·2	11·1	10·2	8·9	7·3	6·0	5·9	7·1	10·1	12·5	12·2	9·8
1873 to 1880	13·4	12·6	11·2	10·2	9·0	7·5	6·3	6·5	7·3	10·3	12·3	13·4	10·0

observations into distinct series at the times 1852·25 and 1866·0; and we have made a further division at 1873·0, because the records after that time are *absolute* ranges for the whole day, whilst those of the preceding years were derived from a register in which the intervals between the hourly observations were all blanks. The means at the foot of Table I are for all the full years of the periods thus separated to form distinct series of observations, and the general means for these periods may, in the absence of immediate comparability of the records of one period with those of another, and proceeding on a very rough hypothesis, be used to reduce the whole series of monthly mean ranges more nearly to a common scale, and thus to prepare them to exhibit, though very doubtfully yet perhaps in the least objectionable manner possible, any long-period variation with which they may be affected.

The rough hypothesis is that the general mean temperature-ranges of the full years of the several periods were really identical, and that the differences found are due to difference in the mode of exposure and in the fulness of the registers kept. This is, of course, objectionable on the ground that if there be a real variation of long-period, it will affect the general means of our four series of observations in different degrees, and any result that we arrive at must accordingly be received with caution; but, on the other hand, the probability of our finding a variation of definite character at all is thereby diminished, and if such a variation should still exhibit itself, it will be in spite of some tendency in our mode of procedure to obliterate it.

3. The general means for the four periods are 11°·8, 11°·2, 9°·8, and 10°·0. Choosing the last as a standard period, the others are reduced to its scale by multiplying the respective monthly mean ranges of Table I by $\frac{10 \cdot 0}{11 \cdot 8}$, $\frac{10 \cdot 0}{11 \cdot 2}$, and $\frac{10 \cdot 0}{9 \cdot 8}$: the values thus obtained, as well as the monthly and annual means—that is, the means of the several horizontal lines and vertical columns—are shown in Table Ia.

Table Ia.—Showing the Monthly and Annual Means of Daily Temperature-Range reduced to a Common Scale.

Year.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Mean of year.	Annual Means smoothed.
1847.........	...	...	...	...	...	6·9	6·2	6·9	7·4	10·3	13·1	13·7		
1848.........	14·9	13·1	11·7	9·7	9·1	6·9	6·3	5·8	8·1	9·3	13·1	13·0	10·1	
1849.........	12·6	11·8	11·2	10·1	8·4	6·9	6·1	5·7	6·1	10·4	12·7	14·0	9·7	
1850.........	12·6	15·4	11·1	9·9	8·7	8·4	6·9	6·9	7·6	10·1	13·5	13·0	10·3	10·03
1851.........	13·4	12·9	11·4	10·8	9·0	7·7	5·7	6·4	8·0	9·6	11·0	13·7	9·9	10·14
1852.........	13·6	12·5	11·9	11·8	10·5	8·3	6·7	6·4	7·1	9·9	13·6	11·5	10·3	10·32
1853.........	14·1	13·4	13·1	11·3	10·6	7·6	5·8	7·6	7·4	11·7	12·8	14·4	10·8	10·45
1854.........	13·8	14·2	12·9	11·1	10·3	8·2	5·8	7·1	7·4	10·3	11·7	10·8	10·3	10·36
1855.........	12·3	11·3	11·4	10·9	10·3	8·0	6·8	8·0	8·2	10·0	12·4	12·6	10·2	10·11
1856.........	12·3	12·1	10·5	10·9	10·0	7·9	5·9	7·0	7·5	9·3	11·9	12·1	9·7	9·92
1857.........	11·3	13·2	12·6	10·9	9·6	8·7	6·6	4·8	6·5	10·0	11·8	13·3	9·9	9·87
1858.........	13·4	12·7	10·5	10·8	9·5	8·4	6·8	6·4	7·4	10·2	12·1	11·7	10·0	9·85
1859.........	12·3	11·5	12·1	10·6	9·2	8·0	7·1	6·1	7·4	9·1	11·3	12·5	9·7	9·75
1860.........	13·2	10·8	10·7	9·0	8·6	8·1	5·3	6·6	6·9	8·9	13·8	13·6	9·6	9·66
1861.........	12·8	13·7	11·2	10·1	9·6	7·9	5·0	4·8	7·2	8·4	12·1	12·6	9·6	9·69
1862.........	12·1	13·1	11·9	10·3	9·4	7·4	6·6	6·9	7·0	9·1	11·4	13·6	9·9	9·80
1863.........	13·0	14·7	10·5	10·3	9·5	6·6	6·3	6·0	7·5	9·8	12·5	12·4	9·9	9·89
1864.........	13·5	12·5	11·6	9·7	9·3	7·9	5·7	6·3	8·2	10·8	11·3	13·1	10·0	9·91
1865.........	13·0	11·9	11·3	10·1	9·1	8·5	6·3	5·6	8·2	9·9	12·1	11·2	9·7	9·97
1866.........	13·7	14·0	11·0	11·1	9·4	7·1	6·0	5·7	8·1	10·8	14·3	13·6	10·4	10·08
1867.........	13·3	14·0	11·8	9·7	8·9	7·4	6·1	5·7	6·9	10·2	12·6	13·5	10·0	10·13
1868.........	13·3	13·1	11·2	10·1	9·3	7·4	6·5	6·0	7·9	11·1	13·9	13·7	10·3	10·03
1869.........	13·3	12·7	10·8	10·2	9·4	7·4	6·4	6·2	6·7	9·7	12·3	10·8	9·7	9·85
1870.........	12·8	13·2	11·1	10·7	8·9	7·7	5·4	6·4	7·1	9·1	11·0	12·2	9·6	9·77
1871.........	13·1	12·7	11·9	10·4	8·8	7·7	6·6	6·0	7·0	10·6	11·3	12·1	9·9	9·83
1872.........	13·8	14·5	11·3	10·6	9·2	7·4	5·4	6·3	7·2	10·6	13·7	11·3	10·1	9·86
1873.........	13·6	11·4	10·3	9·4	8·6	6·5	4·8	5·8	7·0	11·2	12·3	13·8	9·6	9·77
1874.........	13·5	13·3	11·2	10·0	8·4	7·1	5·4	6·3	6·5	10·4	12·1	12·5	9·7	9·72
1875.........	13·4	11·6	11·0	10·0	8·9	7·2	6·7	6·4	6·1	10·1	12·4	12·6	9·7	9·80
1876.........	13·0	13·1	10·6	9·8	9·0	7·5	5·9	6·5	8·0	11·3	12·3	13·8	10·1	9·94
1877.........	12·3	12·0	11·5	10·6	9·3	8·1	7·4	7·2	8·4	8·8	12·3	10·9	9·9	10·08
1878.........	12·8	13·5	11·3	10·7	9·4	8·4	6·8	6·5	7·7	10·4	10·6	14·1	10·2	10·24
1879.........	14·1	12·6	12·6	10·6	9·0	7·2	7·0	6·0	7·8	10·4	13·8	15·3	10·5	
1880.........	14·6	13·6	11·2	10·4	9·3	8·2	6·7	7·2	7·3	10·1	12·9	13·9	10·4	
Means of columns.	13·18	12·91	11·41	10·38	9·29	7·66	6·21	6·34	7·38	10·06	12·42	12·85	10·0	

4. The annual means in the last column are obtained from those in the next preceding column in the following manner, the object being to remove the irregularities in the progression of the latter numbers and to substitute for those numbers a smooth-flowing series that will retain all the principal characteristics of any variation of long-period that affects them: first, the means are taken of every two adjacent numbers forming a new series the number of which is one less than the number of years; second, the new series of numbers is treated in the same manner, giving a second new series: and the process is repeated a third and a fourth time, the last-resulting series of numbers being those entered in the last column of Table Ia. The annual means and the smoothed annual means are curved in fig. 1, and the latter curve shows distinct maxima in 1853 and 1867, and minima in 1860 or 1861 and between 1870 and 1874, times that imply a rough general correspondence of ebb and flow with that of an inverted sun-spot curve for the same period.

5. The numbers at the foot of Table Ia we adopt as representing

Table II.—Exhibiting the Proportional Temperature-range, the Normal for each Month being reckoned = 100.

Year.	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.	Annual Means.
1847 ..	...	...	...	...	...	90	100	110	100	102	106	107	
1848...	113	102	103	93	98	90	102	92	109	92	106	102	100·2
1849...	95	91	98	97	90	90	98	90	82	103	102	109	95·4
1850...	95	119	97	95	94	109	111	110	103	100	109	102	103·7
1851...	102	100	100	104	97	100	92	102	108	95	89	107	99·7
1852...	103	97	104	113	113	108	108	102	96	98	110	90	103·5
1853...	107	104	115	109	114	99	94	121	100	116	103	112	107·8
1854...	105	110	113	107	111	106	94	113	100	102	94	84	103·2
1855...	93	88	100	105	111	104	110	127	111	99	100	98	103·8
1856...	93	94	92	105	108	103	95	111	101	92	96	95	98·7
1857...	86	102	110	105	103	113	106	76	88	99	95	104	98·9
1858...	102	98	92	104	102	109	110	102	100	101	98	91	100·7
1859...	93	89	106	102	99	104	115	97	100	90	91	98	98·7
1860...	100	84	94	87	92	105	85	105	93	88	111	106	95·8
1861...	97	106	98	97	103	103	81	76	97	83	98	98	94·7
1862...	92	102	104	99	101	96	106	110	95	90	92	106	99·4
1863...	98	114	92	99	102	86	102	95	101	97	101	97	98·7
1864...	102	97	102	93	100	103	92	100	111	107	91	102	100·0
1865...	98	92	99	97	98	108	102	89	111	98	98	87	98·1
1866...	104	109	96	107	101	92	97	90	109	107	115	106	102·7
1867...	101	109	104	93	96	96	98	90	93	101	102	105	99·0
1868...	101	102	98	97	100	96	105	95	107	110	112	107	102·5
1869...	101	98	95	98	101	96	103	98	91	96	99	84	96·7
1870...	97	102	97	103	96	100	87	102	96	90	89	95	96·2
1871...	99	98	104	100	95	100	106	95	95	105	91	95	98·6
1872...	104	112	99	102	99	96	87	100	97	105	110	88	99·9
1873...	103	88	90	90	92	84	77	92	95	111	99	108	94·1
1874...	102	103	98	96	90	92	87	100	88	103	98	98	96·2
1875...	102	89	96	96	96	94	108	102	82	100	100	98	96·9
1876...	98	102	93	94	97	97	95	103	108	112	99	108	100·5
1877...	93	93	101	102	100	105	119	114	114	87	99	85	101·0
1878...	97	105	99	103	101	110	110	103	104	103	85	110	102·5
1879...	107	98	111	102	97	94	113	95	105	103	111	120	104·7
1880...	111	105	98	100	100	106	108	114	99	100	104	109	104·5

the annual variation, combined with the annual mean value of temperature-range, on the scale of the period 1873 to 1880.

6. Arranging now the results in the manner adopted by Dr. Stewart in following out his hypothesis that solar activity varies with the temperature-range, being inversely proportional to the mean range in different months of the typical year and directly proportional to the range in the same month of different years, we convert the several monthly numbers of Table Ia into corresponding percentages of solar activity, by multiplying them by 100 and dividing by the mean temperature-range of all the years for the corresponding month. Table II has been formed in this manner.

The numbers in the last column vary, of course, in the same manner as, though not exactly proportionally to, the annual means of Table Ia.

C. *Lunar Annual Variation.*

7. The purpose of this section and the process by which it is served may be described in Dr. Stewart's own words, which are as follows:—

"It will be of interest to determine whether the temperature-range has any reference to the relative position of the sun and moon. For this purpose the whole period of observation has been portioned out into lunations, beginning with new moon. Each lunation is divided into eight parts, entitled—(0), (1), (2), (3), (4), (5), (6), (7),—(0) denoting new, and (4) full moon.

"The various lunations with the corresponding values of the temperature-range are exhibited in Table III. It will, however, be here necessary to state how these values have been obtained. Take the dates (civil time) of the four quarterly phases of the moon as given by the "Nautical Almanac," and under each of these dates, as a centre, group seven observations. Each value in Table III corresponding to (0), (2), (4), (6) is thus the mean of seven separate observations of daily range.

"The half-quarterly phases (1), (3), (5), (7) are then interpolated in point of time, so that sometimes their date will fall upon a given civil day, and sometimes between one civil day and another. In the former case the mean of seven observations, and in the latter the mean of six, is taken."

The only special point that needs mention is, that only the observations of the 33 full years 1847 ·75 to 1880 ·75, including 33 complete winters and 33 complete summers, are made use of in the calculations of lunar variations, those of the portions of a summer or winter, 1847 ·42 to 1847 ·75 and 1880 ·75 to 1881 ·0, being omitted.

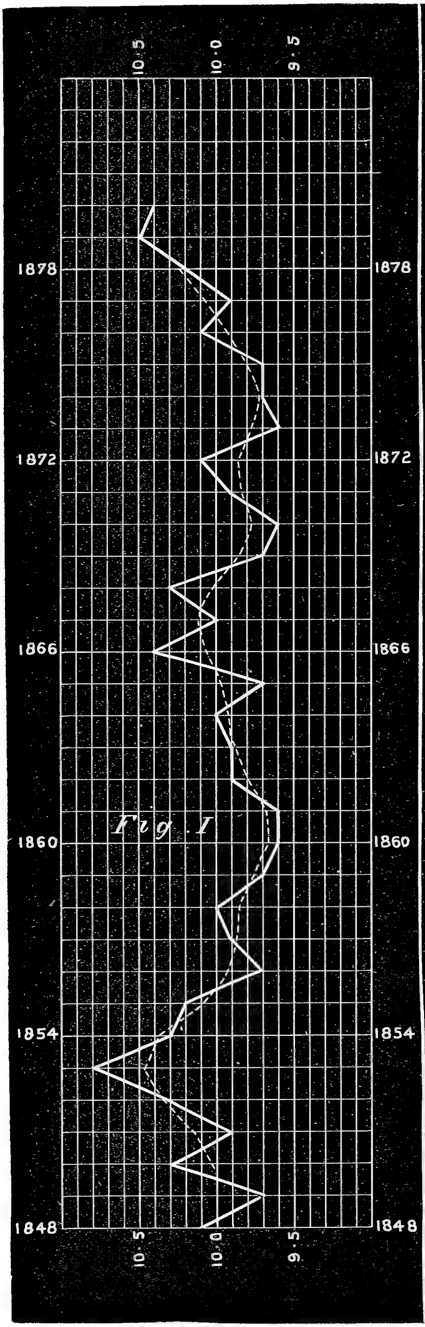
Fig. I
10·5
10·0
9·5
1878
1872
1866
1860
1854
1848

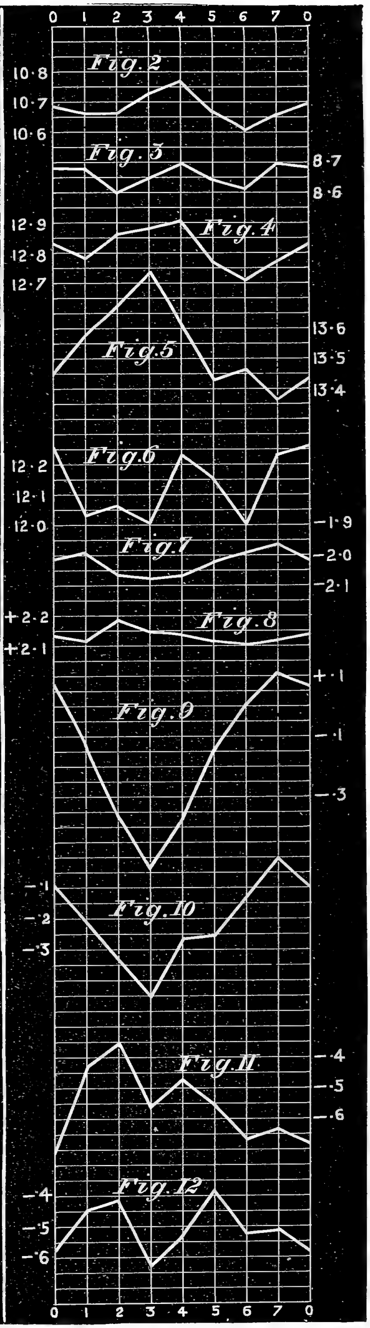
0 1 2 3 4 5 6 7 0
Fig. 2
10·8
10·7
10·6
Fig. 3
8·7
8·6
Fig. 4
12·9
12·8
12·7
Fig. 5
13·6
13·5
13·4
Fig. 6
12·2
12·1
12·0
Fig. 7
−1·9
−2·0
−2·1
Fig. 8
+2·2
+2·1
Fig. 9
+·1
−·1
−·3
Fig. 10
−·1
−·2
−·3
Fig. 11
−·4
−·5
−·6
Fig. 12
−·4
−·5
−·6

Table III.—Exhibiting the Temperature-Ranges grouped according to Lunations.

No. of lunation.	Date of new moon beginning lunation.	(0).	(1).	(2).	(3).	(4).	(5).	(6).	(7).
1	Sept. 9, 1847	7·0°	8·2°	8·8°	10·3	10·1°	9·7°	9·1°	10·6°
2	Oct. 8 ,,	12·6	14·1	11·2	11·4	11·9	13·7	13·0	9·0
3	Nov. 7 ,,	12·5	16·6	19·1	19·1	17·7	14·5	15·5	17·9
4	Dec. 7 ,,	17·0	15·5	17·7	16·1	11·2	15·0	18·1	17·5
5	Jan. 6, 1848	17·3	18·6	19·8	20·2	18·2	16·6	14·9	15·3
6	Feb. 4 ,,	16·7	17·1	15·3	15·2	16·7	15·9	13·1	12·9
7	Mar. 5 ,,	14·6	15·7	15·8	13·2	14·3	12·6	12·6	12·0
(8)	April 3 ,,	13·3	12·6	10·2	10·4	12·4	11·4	11·0	10·2
(9)	May 2 ,,	10·3	10·0	9·8	10·4	10·1	9·9	11·6	11·8
(10)	June 1 ,,	9·2	8·3	9·0	9·6	7·6	5·7	7·2	8·3
(11)	June 30 ,,	9·0	8·7	8·7	8·7	7·5	5·5	6·0	7·2
(12)	July 29 ,,	6·9	6·2	6·5	6·5	7·1	6·8	6·4	8·0
(13)	Aug. 28 ,,	7·3	7·6	8·6	9·2	10·3	10·4	10·6	9·6
14	Sept. 26 ,,	8·6	10·6	10·1	8·3	9·3	9·1	9·5	12·1
15	Oct. 26 ,,	14·6	14·8	14·0	14·7	16·0	15·8	14·6	12·4
16	Nov. 25 ,,	15·9	18·6	16·7	14·7	15·5	14·7	13·6	16·0
17	Dec. 25 ,,	17·0	14·1	15·2	15·9	16·5	16·0	14·1	16·7
18	Jan. 23, 1849	13·9	12·5	12·2	15·2	16·7	13·1	11·7	11·9
19	Feb. 22 ,,	12·6	14·2	15·7	14·4	12·9	10·9	12·9	13·3
(20)	Mar. 24 ,,	12·9	13·7	14·6	13·3	13·3	13·7	11·5	10·8
(21)	April 22 ,,	11·1	10·5	9·2	9·1	10·9	10·0	9·8	10·1
(22)	May 21 ,,	10·2	10·3	9·1	10·1	9·2	8·5	8·5	7·5
(23)	June 20 ,,	6·6	6·9	7·6	7·4	6·0	7·2	7·5	9·0
(24)	July 19 ,,	9·4	8·3	6·1	4·5	5·2	5·9	6·4	6·6
(25)	Aug. 17 ,,	6·9	7·5	7·0	7·9	7·9	8·3	6·7	5·8
26	Sept. 16 ,,	5·2	6·2	9·2	9·5	11·8	12·8	9·9	9·1
27	Oct. 15 ,,	10·0	11·3	15·1	17·4	14·9	12·3	13·2	13·0
28	Nov. 14 ,,	14·1	15·6	15·8	18·8	17·5	17·2	17·6	17·3
29	Dec. 14 ,,	17·2	15·2	16·1	16·9	15·6	13·6	13·9	14·3
30	Jan. 13, 1850	14·2	13·7	15·7	17·7	15·9	13·9	18·9	20·3
31	Feb. 11 ,,	18·8	14·4	16·3	19·4	18·3	19·6	16·2	13·9
(32)	Mar. 13 ,,	11·3	10·3	11·8	12·8	12·5	9·5	10·2	12·3
(33)	April 12 ,,	11·3	11·6	11·5	12·6	13·1	11·8	12·0	11·4
(34)	May 11 ,,	9·6	9·4	10·2	10·3	10·0	9·6	9·4	9·9
(35)	June 9 ,,	10·6	9·7	9·4	9·5	9·5	10·8	10·4	9·4
(36)	July 9 ,,	9·3	8·5	7·2	7·6	5·6	7·0	8·4	7·4
(37)	Aug. 7 ,,	7·1	7·4	8·6	7·9	8·2	9·2	8·9	8·8
(38)	Sept. 5 ,,	8·4	8·2	8·7	9·1	6·2	9·4	9·7	9·9
39	Oct. 5 ,,	11·3	11·6	12·4	13·5	15·0	13·7	9·5	10·0
40	Nov. 3 ,,	12·6	13·5	15·7	17·0	17·6	15·9	17·0	16·8
41	Dec. 3 ,,	15·0	14·8	14·8	15·9	15·0	14·4	16·3	16·5
42	Jan. 1, 1851	15·9	13·9	13·5	16·2	15·7	13·1	19·2	15·5
43	Jan. 31 ,,	16·2	13·7	12·8	12·7	14·0	17·8	18·8	16·0
44	Mar. 2 ,,	12·8	13·0	13·6	14·3	13·3	13·3	12·8	13·3
(45)	April 1 ,,	14·4	12·5	12·4	12·3	13·3	13·7	11·5	12·1
(46)	April 30 ,,	12·3	10·2	10·3	11·3	10·9	9·9	10·2	11·4
(47)	May 30 ,,	10·5	9·7	9·1	7·1	8·5	9·6	10·3	9·5
(48)	June 28 ,,	8·0	8·8	8·9	7·9	6·1	5·1	6·4	6·7
(49)	July 28 ,,	5·2	6·2	6·8	7·1	8·7	8·1	7·5	6·7
(50)	Aug. 26 ,,	6·2	7·3	8·1	8·7	9·1	9·5	10·6	11·0
51	Sept. 24 ,,	11·2	9·1	8·1	10·3	10·5	10·5	10·4	11·3
52	Oct. 24 ,,	12·7	13·8	14·8	14·9	12·1	10·9	12·4	12·0
53	Nov. 22 ,,	12·4	14·0	13·7	14·1	16·5	17·0	17·6	17·3

No. of lunation.	Date of new moon beginning lunation.	(0).	(1).	(2).	(3).	(4).	(5).	(6).	(7).
54	Dec. 22, 1851	16·9°	15·1°	16·0°	18·3°	14·6°	14·2°	15·7°	15·5°
55	Jan. 21, 1852	16·9	18·6	14·0	14·2	15·7	16·3	15·5	14·3
56	Feb. 20 ,,	15·6	13·9	12·6	12·6	15·7	19·1	15·5	13·0
(57)	Mar. 21 ,,	12·6	12·5	12·9	13·0	14·4	14·0	12·7	12·8
(58)	April 19 ,,	12·6	13·0	13·0	12·8	13·4	12·7	11·1	11·5
(59)	May 19 ,,	11·5	11·4	11·3	11·8	13·9	12·1	10·9	10·5
(60)	June 17 ,,	8·5	6·1	6·3	6·7	7·1	6·7	7·5	7·5
(61)	July 17 ,,	7·7	8·9	8·4	5·8	6·2	6·3	4·3	4·5
(62)	Aug. 15 ,,	6·2	7·9	8·8	9·4	9·5	9·2	8·8	8·6
63	Sept. 14 ,,	7·1	6·5	7·2	8·4	8·1	9·4	11·1	10·6
64	Oct. 13 ,,	9·1	9·4	11·1	12·7	13·9	14·2	14·7	14·8
65	Nov. 11 ,,	15·3	16·0	15·1	14·2	16·4	15·5	11·0	6·7
66	Dec. 11 ,,	11·0	14·7	15·7	16·0	13·9	13·0	15·6	16·0
67	Jan. 9, 1853	15·1	15·2	16·0	16·4	16·5	16·2	16·3	15·5
68	Feb. 8 ,,	15·9	16·0	14·7	13·4	13·8	13·9	15·8	15·7
(69)	Mar. 10 ,,	14·8	14·7	14·7	14·1	15·1	13·1	12·3	12·3
(70)	April 8 ,,	12·9	12·6	12·6	12·8	13·0	12·3	12·0	12·3
(71)	May 8 ,,	12·5	12·2	11·5	11·3	11·7	11·9	11·2	10·5
(72)	June 7 ,,	11·9	11·1	7·3	6·5	5·4	6·3	8·1	7·7
(73)	July 6 ,,	6·9	5·7	6·8	6·6	5·5	6·1	6·7	7·5
(74)	Aug. 5 ,,	8·0	9·4	9·8	9·2	9·2	8·8	8·9	5·5
(75)	Sept. 3 ,,	4·6	6·9	7·7	9·3	9·9	10·4	9·2	9·0
76	Oct. 3 ,,	10·7	10·2	11·3	12·6	14·1	15·6	14·8	13·6
77	Nov. 1 ,,	15·0	14·7	14·5	15·5	13·3	12·7	14·2	15·5
78	Dec. 1 ,,	15·3	16·0	16·8	16·3	14·1	14·8	16·4	18·4
79	Dec. 30 ,,	17·7	16·3	15·2	14·9	15·2	16·8	16·4	14·9
80	Jan. 28, 1854	14·6	15·8	18·4	17·2	14·2	16·0	15·5	14·2
81	Feb. 27 ,,	14·3	14·0	16·0	16·5	14·3	14·7	14·5	14·1
(82)	Mar. 28 ,,	13·5	13·1	12·3	12·2	12·5	13·0	13·0	12·6
(83)	April 27 ,,	11·7	11·5	11·7	11·8	11·7	11·7	11·1	10·6
(84)	May 27 ,,	11·4	10·7	11·5	10·6	8·3	9·2	9·7	8·5
(85)	June 25 ,,	8·0	7·3	6·5	7·0	9·1	8·0	5·9	5·7
(86)	July 25 ,,	5·9	6·0	5·3	5·8	6·9	8·2	8·4	8·9
(87)	Aug. 23 ,,	9·2	8·3	7·3	7·3	6·5	4·8	6·5	9·5
88	Sept. 22 ,,	9·6	10·6	11·3	11·1	9·4	9·0	9·9	12·3
89	Oct. 22 ,,	14·2	14·4	12·0	9·8	11·6	14·9	13·9	12·4
90	Nov. 20 ,,	14·4	15·6	12·9	9·8	11·0	13·1	12·6	12·0
91	Dec. 20 ,,	11·5	11·6	12·4	12·8	12·6	12·1	12·6	13·7
92	Jan. 18, 1855	16·8	15·6	14·1	15·0	14·2	12·4	12·3	12·4
93	Feb. 17 ,,	11·7	11·3	12·4	12·5	11·7	12·1	13·2	13·4
(94)	Mar. 18 ,,	13·0	14·4	13·6	11·9	11·8	12·5	12·8	12·7
(95)	April 16 ,,	12·0	12·3	11·9	11·6	11·8	11·6	11·9	11·9
(96)	May 16 ,,	10·8	10·3	11·6	12·0	11·8	10·5	9·2	9·8
(97)	June 14 ,,	9·9	8·7	8·3	8·5	6·1	6·0	7·5	6·4
(98)	July 14 ,,	6·8	8·0	8·6	9·0	9·1	8·7	7·9	8·6
(99)	Aug. 13 ,,	10·1	9·3	9·3	9·5	9·8	9·0	8·5	7·6
100	Sept. 11 ,,	8·2	9·5	9·6	9·9	10·2	10·5	10·3	11·8
101	Oct. 11 ,,	12·9	10·6	10·2	10·5	10·2	11·8	14·8	15·0
102	Nov. 10 ,,	12·5	12·5	13·2	14·1	13·5	13·6	14·4	12·9
103	Dec. 9 ,,	10·7	11·6	13·9	16·0	16·4	15·7	17·2	15·7
104	Jan. 8, 1856	14·0	14·3	14·4	14·2	12·8	11·0	13·4	15·3
105	Feb. 6 ,,	13·7	12·6	12·5	14·5	14·3	12·1	12·6	11·1
(106)	Mar. 7 ,,	11·5	11·9	11·5	12·0	11·9	12·4	12·4	11·9
(107)	April 5 ,,	12·1	12·9	12·5	12·7	12·8	12·0	11·3	10·6
(108)	May 4 ,,	11·0	11·5	10·9	10·8	11·6	12·5	10·5	11·2
(109)	June 3 ,,	9·8	8·1	8·5	8·6	8·6	9·2	8·7	8·3
(110)	July 2 ,,	6·2	6·7	7·8	7·5	7·6	6·3	5·5	6·3

No. of lunation.	Date of new moon beginning lunation.	(0).	(1).	(2).	(3).	(4).	(5).	(6).	(7).
(111)	Aug. 1, 1856	6·8°	6·5°	7·8°	8·3°	8·8°	8·5°	8·1°	8·2°
(112)	Aug. 31 ,,	7·5	7·5	9·0	9·8	8·9	6·4	6·6	9·2
113	Sept. 29 ,,	9·5	9·3	10·6	10·4	10·9	10·7	10·2	10·2
114	Oct. 29 ,,	11·3	10·8	10·8	12·8	15·7	15·8	14·4	12·5
115	Nov. 27 ,,	12·8	13·9	13·8	16·1	14·3	12·6	13·3	13·7
116	Dec. 27 ,,	13·2	10·2	9·0	10·7	12·1	12·6	14·1	14·0
117	Jan. 26, 1857	13·6	15·0	13·6	11·9	14·9	15·0	15·9	15 6
118	Feb. 24 ,,	15·0	16·0	14·4	13·8	13·9	12·5	15·7	14·3
(119)	Mar. 26 ,,	13·7	13·2	13·1	12·9	12·8	11·5	10·4	12·4
(120)	April 24 ,,	13·5	12·2	10·5	9·9	10·8	11·0	11·0	10·5
(121)	May 23 ,,	10·6	12·4	12·2	11·2	10·5	11·0	10·6	9·9
(122)	June 22 ,,	8·8	6·9	7·4	8·4	7·7	7·8	8·1	7·9
(123)	July 21 ,,	7·6	6·5	4·7	4·6	4·1	3·6	5·3	6·3
(124)	Aug. 19 ,,	5·1	6·0	6·8	7·4	7·7	6·1	6·6	7·3
125	Sept. 18 ,,	5·4	7·0	9·0	9·4	9·1	9·5	11·2	11·5
126	Oct. 18 ,,	10·9	11·4	13·0	12·4	12·4	11·3	11·3	12·9
127	Nov. 16 ,,	14·2	15·0	14·6	13·0	13·9	15·0	14·1	13·6
128	Dec. 16 ,,	13·6	15·5	15·5	16·5	17·1	15·8	14·7	16·2
129	Jan. 15, 1858	16·1	15·5	13·9	12·0	14·6	13·5	13·6	15·0
130	Feb. 14 ,,	15·9	15·6	13·6	13·7	11·5	10·6	10·5	11·2′
(131)	Mar. 15 ,,	11·8	12·3	12·4	11·9	12·9	12·8	12·3	11·8
(132)	April 14 ,,	12·2	12·0	12·0	12·3	12·1	11·5	10·2	10·1
(133)	May 13 ,,	10·9	10·6	10·2	10·9	11·2	10·2	10·4	10·9
(134)	June 11 ,,	10·3	10·4	9·9	8·0	7·2	8·0	8·5	8·2
(135)	July 11 ,,	6·8	6·1	7·3	8·2	7·1	6·6	6·7	6·5
(136)	Aug. 9 ,,	7·2	8·0	8·2	8·0	7·2	6·9	8·8	9·8
(137)	Sept. 7 ,,	9·4	7·4	7·1	8·1	8·3	8·4	9·0	10·0
138	Oct. 7 ,,	11·0	10·4	9·2	10·7	12·4	13·6	14·0	14·2
139	Nov. 5 ,,	14·0	13·7	15·1	14·9	13·5	11·6	10·9	12·6
140	Dec. 5 ,,	11·6	12·1	14·2	12·4	14·0	14·6	13·7	11·2
141	Jan. 4, 1859	13·7	15·9	14·2	14·1	15·7	14·9	11·2	12·1
142	Feb. 3 ,,	13·7	12·1	14·9	17·0	15·7	10·4	10·9	10·4
143	Mar. 5 ,,	10·8	11·1	12·2	13·9	14·8	14·5	12·0	12·5
(144)	April 3 ,,	13·0	12·7	12·1	12·0	11·9	11·2	11·4	11·6
(145)	May 3 ,,	11·3	11·3	9·9	8·6	9·9	11·1	10·1	9·9
(146)	June 1 ,,	11·3	10·2	8·6	9·0	8·4	8·5	8·2	8·2
(147)	June 30 ,,	9·2	9·7	9·7	10·4	9·3	5·9	5·3	5·8
(148)	July 30 ,,	5·0	5·8	7·0	8·6	8·3	8·7	6·7	3·8
(149)	Aug. 28 ,,	5·0	7·4	7·2	7·1	7·6	8·2	8·2	9·2
150	Sept. 26 ,,	10·1	10·2	9·3	8·6	9·5	9·6	9·9	10·3
151	Oct. 26 ,,	11·7	13·3	12·9	12·6	12·8	11·5	11·8	13·7
152	Nov. 24 ,,	13·3	12·7	13·3	15·9	13·9	13·3	14·1	14·0
153	Dec. 24 ,,	14·0	15·3	15·7	15·0	15·3	16·2	14·5	13·2
154	Jan. 23, 1860	13·9	15·7	14·1	13·1	12·3	13·8	13·1	11·4
155	Feb. 22 ,,	9·9	10·8	12·4	13·4	12·7	11·1	10·3	10·8
(156)	Mar. 22 ,,	12·3	12·5	12·6	12·0	11·5	11·5	10·5	9·9
(157)	April 21 ,,	10·0	9·0	8·2	10·4	11·0	10·2	9·8	9·4
(158)	May 21 ,,	7·6	8·1	9·5	10·3	10·0	9·9	9·7	7·4
(159)	June 19 ,,	9·1	10·1	9·4	7·7	5·7	5·5	6·3	6·5
(160)	July 18 ,,	5·6	5·8	6·3	5·7	5·4	6·6	7·6	7·1
(161)	Aug. 17 ,,	6·9	7·4	8·4	8·1	8·4	6·7	5·4	6·1
162	Sept. 15 ,,	7·5	9·1	9·6	8·7	8·0	8·6	9·4	9·6
163	Oct. 14 ,,	11·5	11·5	10·8	10·3	10·9	14·0	15·8	15·7
164	Nov. 13 ,,	15·6	16·3	14·8	14·3	16·5	17·3	16·2	16·4
165	Dec. 12 ,,	12·7	11·7	14·4	16·4	15·6	14·2	14·0	12·1
166	Jan. 11, 1861	13·6	12·4	13·0	17·9	16·4	14·6	14·7	12·4
167	Feb. 10 ,,	12·4	13·1	19·1	17·8	15·9	14·8	12·1	14·1

No. of lunation.	Date of new moon beginning lunation.	(0).	(1).	(2).	(3).	(4).	(5).	(6).	(7).
(168)	Mar. 11, 1861	15·9°	13·9°	12·1°	12·1°	12·0°	12·2°	12·0°	12·1°
(169)	April 10 ,,	12·1	11·6	11·2	10·8	10·3	10·8	11·5	12·4
(170)	May 10 ,,	11·0	9·6	9·8	10·6	10·7	11·1	11·4	9·7
(171)	June 8 ,,	9·6	10·0	9·5	7·4	7·6	7·8	6·0	4·6
(172)	July 8 ,,	6·5	7·3	6·2	5·5	5·3	5·5	5·9	6·9
(173)	Aug. 6 ,,	7·4	6·8	5·1	3·5	4·7	5·5	5·6	5·6
(174)	Sept. 5 ,,	6·0	7·2	9·2	9·1	8·6	8·4	8·4	7·6
175	Oct. 4 ,,	8·1	9·8	9·4	8·3	8·5	9·9	10·7	12·5
176	Nov. 2 ,,	14·1	14·7	13·3	11·3	11·7	14·4	14·7	14·2
177	Dec. 2 ,,	15·1	14·8	14·0	13·2	13·1	14·4	13·6	14·3
178	Dec. 31 ,,	15·3	14·6	11·6	13·0	14·4	14·1	13·2	15·3
179	Jan. 30, 1862	14·0	16·6	18·8	15·0	12·0	13·9	15·1	13·9
180	Feb. 28 ,,	12·5	12·9	12·4	14·6	14·3	13·9	12·8	12·4
(181)	Mar. 30 ,,	12·3	11·4	10·8	11·5	11·2	11·0	11·9	11·9
(182)	April 29 ,,	11·6	11·3	10·4	10·0	10·8	11·2	10·3	9·5
(183)	May 28 ,,	10·0	9·5	9·7	9·7	6·7	6·4	8·6	8·8
(184)	June 27 ,,	8·5	6·7	7·8	8·3	7·7	7·5	7·9	7·7
(185)	July 27 ,,	6·4	6·3	7·6	7·9	7·9	6·9	6·5	8·1
(186)	Aug. 25 ,,	8·1	8·2	8·1	7·7	8·4	8·1	8·1	7·4
187	Sept. 24 ,,	8·2	7·2	6·8	8·1	9·0	9·1	9·4	11·6
188	Oct. 23 ,,	12·3	11·7	12·4	12·8	13·5	13·2	14·6	14·8
189	Nov. 21 ,,	12·0	9·6	11·5	12·2	13·6	15·2	15·2	15·5
190	Dec. 21 ,,	17·0	17·9	15·9	15·3	15·6	16·0	14·9	13·8
191	Jan. 19, 1863	13·1	14·1	15·1	13·3	14·1	14·4	14·9	15·2
192	Feb. 18 ,,	16·3	19·4	20·4	15·3	11·5	10·5	11·9	13·5
(193)	Mar. 19 ,,	13·5	12·0	11·2	11·0	11·4	12·9	12·7	11·5
(194)	April 18 ,,	11·6	11·7	10·5	10·4	10·7	10·7	11·0	10·4
(195)	May 17 ,,	10·5	11·3	10·4	10·4	10·2	9·5	8·0	7·0
(196)	June 16 ,,	6·7	5·8	6·4	7·1	8·1	8·0	6·5	7·3
(197)	July 16 ,,	8·2	7·8	6·3	5·8	5·9	6·1	7·1	6·0
(198)	Aug. 14 ,,	5·5	7·0	7·2	7·1	7·5	7·8	7·1	7·4
199	Sept. 13 ,,	9·0	9·8	9·3	8·5	8·4	9·7	10·0	10·0
200	Oct. 13 ,,	11·1	11·6	11·8	11·1	10·2	12·5	13·3	13·1
201	Nov. 11 ,,	14·0	13·7	14·5	14·2	15·0	15·3	16·6	12·0
202	Dec. 11 ,,	11·3	13·3	14·0	14·9	15·0	13·6	13·4	14·4
203	Jan. 9, 1864	16·1	14·3	14·9	14·5	15·1	16·0	15·1	15·7
204	Feb. 7 ,,	17·7	16·5	12·8	11·2	12·7	13·4	13·7	14·8
(205)	Mar. 8 ,,	13·4	12·3	12·7	12·7	12·2	12·3	11·2	11·4
(206)	April 6 ,,	11·9	10·2	10·7	11·8	11·9	10·5	9·1	10·0
(207)	May 6 ,,	10·0	9·6	9·9	10·7	10·1	11·0	11·3	11·0
(208)	June 4 ,,	10·2	11·0	10·9	10·4	7·8	6·0	5·9	6·8
(209)	July 4 ,,	8·2	7·3	6·6	5·3	4·1	4·7	6·7	7·8
(210)	Aug. 2 ,,	6·8	6·6	6·6	6·2	7·5	8·6	7·2	6·7
(211)	Sept. 1 ,,	6·5	7·4	8·6	8·8	9·1	9·6	9·5	11·7
212	Oct. 1 ,,	12·1	11·4	11·8	11·4	12·2	14·3	11·9	12·7
213	Oct. 30 ,,	13·8	11·6	11·0	11·9	14·8	16·1	13·1	13·1
214	Nov. 29 ,,	11·5	11·4	13·8	15·2	16·7	15·8	14·8	15·1
215	Dec. 29 ,,	14·0	14·4	14·3	15·0	16·2	16·4	15·2	14·2
216	Jan. 27, 1865	13·6	11·2	12·1	12·1	12·4	15·4	13·5	13·4
217	Feb. 26 ,,	13·1	12·1	11·2	11·9	14·4	14·5	13·0	12·0
(218)	Mar. 27 ,,	12·1	12·5	12·6	12·3	11·4	11·4	11·6	10·7
(219)	April 25 ,,	9·7	9·8	10·3	11·3	10·8	8·6	8·4	10·2
(220)	May 25 ,,	10·8	10·6	9·5	11·3	10·5	8·4	8·4	9·9
(221)	June 23 ,,	9·4	9·1	8·2	5·1	5·2	7·2	7·7	7·5
(222)	July 22 ,,	7·8	7·9	8·1	8·4	8·3	7·9	5·2	4·2
(223)	Aug. 21 ,,	4·1	5·6	7·0	7·3	8·3	8·5	7·1	8·7
224	Sept. 20 ,,	10·9	10·5	10·3	10·3	10·5	10·9	10·9	11·4

No. of lunation.	Date of new moon beginning lunation.	(0).	(1).	(2).	(3).	(4).	(5).	(6).	(7).
225	Oct. 19, 1865	12·7°	12·2°	10·6°	10·2°	10·3°	11·9°	13·5°	15·4°
226	Nov. 18 ,,	15·6	14·3	14·6	12·3	10·3	10·1	11·3	14·5
227	Dec. 18 ,,	13·8	13·2	14·3	15·1	13·4	13·0	10·4	12·4
228	Jan. 17, 1866	14·1	14·5	13·7	12·3	15·3	13·9	11·8	15·3
229	Feb. 15 ,,	13·8	12·5	14·6	12·9	12·1	10·2	10·8	11·1
(230)	Mar. 17 ,,	10·9	9·5	10·2	12·1	11·4	11·5	11·5	11·6
(231)	April 15 ,,	11·6	11·6	10·8	9·6	9·2	10·0	9·7	9·7
(232)	May 14 ,,	9·4	8·7	8·3	8·9	8·9	8·5	8·9	9·0
(233)	June 13 ,,	7·6	5·0	4·4	5·9	6·4	6·6	6·6	5·8
(234)	July 12 ,,	6·1	7·1	6·3	5·1	5·4	3·7	4·6	4·7
(235)	Aug. 10 ,,	3·9	5·2	6·1	6·4	7·1	7·3	6·5	6·6
236	Sept. 9 ,,	7·4	7·4	8·5	8·1	8·4	9·4	9·0	8·9
237	Oct. 8 ,,	9·0	9·6	10·5	11·0	12·0	13·5	14·3	13·3
238	Nov. 7 ,,	12·8	14·0	14·8	14·3	13·2	13·8	14·8	14·9
239	Dec. 7 ,,	14·3	12·9	11·6	11·6	12·4	14·3	14·8	12·3
240	Jan. 6, 1867	12·4	14·0	13·5	13·4	13·0	12·9	12·9	13·0
241	Feb. 4 ,,	14·2	14·1	13·0	13·5	13·7	15·2	13·3	12·2
(242)	Mar. 6 ,,	13·3	13·2	11·5	10·7	9·5	10·4	11·8	10·6
(243)	April 5 ,,	9·4	9·6	9·9	8·9	9·1	9·8	9·8	8·9
(244)	May 4 ,,	8·7	8·7	8·8	9·1	9·1	8·6	8·2	8·5
(245)	June 2 ,,	8·6	8·3	7·8	7·5	8·0	6·2	5·7	6·5
(246)	July 2 ,,	5·6	5·5	7·7	7·3	4·7	5·5	6·4	5·6
(247)	July 31 ,,	4·5	5·1	5·1	4·6	4·3	5·4	6·9	7·0
(248)	Aug. 29 ,,	7·1	6·7	6·3	6·9	6·2	5·6	6·6	7·8
249	Sept. 28 ,,	8·1	8·4	8·6	10·4	11·0	10·4	9·0	10·1
250	Oct. 27 ,,	11·5	11·5	13·4	12·7	13·0	13·4	12·7	13·1
251	Nov. 26 ,,	10·1	9·7	13·5	14·0	12·6	12·0	13·0	12·0
252	Dec. 26 ,,	13·7	14·9	12·9	13·5	13·1	12·7	13·4	13·1
253	Jan. 25, 1868	12·7	12·6	13·5	11·8	12·0	10·7	11·7	11·7
254	Feb. 23 ,,	12·5	15·4	12·7	12·6	11·5	10·1	11·3	11·7
(255)	Mar. 24 ,,	10·8	10·2	10·2	11·6	11·5	9·8	9·9	10·1
(256)	April 23 ,,	9·8	8·7	8·2	9·4	9·8	9·8	9·3	8·9
(257)	May 22 ,,	9·4	8·6	7·8	8·5	9·1	6·3	5·6	6·3
(258)	June 20 ,,	6·2	7·3	8·4	7·8	7·4	8·0	5·8	5·9
(259)	July 20 ,,	6·1	6·3	5·5	5·7	6·9	5·8	4·2	4·6
(260)	Aug. 18 ,,	5·3	6·6	7·4	7·1	6·9	7·1	6·5	7·5
261	Sept. 16 ,,	8·1	7·6	7·7	8·3	8·8	8·5	10·1	13·3
262	Oct. 16 ,,	13·2	10·6	11·1	10·8	10·4	11·0	13·2	13·2
263	Nov. 14 ,,	14·2	15·8	15·1	14·0	14·0	14·5	14·8	14·3
264	Dec. 14 ,,	12·7	12·9	13·1	12·5	12·6	12·4	13·0	13·8
265	Jan. 13, 1869	13·2	13·8	13·9	12·3	11·1	11·0	11·4	12·6
266	Feb. 11 ,,	15·0	12·1	11·3	12·7	13·0	10·8	10·0	12·1
(267)	Mar. 13 ,,	11·6	10·7	9·9	9·4	9·7	10·4	10·8	10·2
(268)	April 12 ,,	9·8	10·4	9·6	8·5	9·7	11·3	10·7	9·3
(269)	May 11 ,,	9·4	9·7	8·8	8·5	8·4	8·4	8·6	7·5
(270)	June 10 ,,	7·9	8·8	7·8	6·3	6·2	5·8	6·4	6·8
(271)	July 9 ,,	7·4	5·9	5·3	4·1	4·9	6·6	7·0	6·8
(272)	Aug. 8 ,,	6·2	6·6	7·5	6·1	5·0	5·8	5·5	5·4
(273)	Sept. 6 ,,	6·4	7·4	7·8	6·8	5·8	6·5	5·8	6·0
274	Oct. 5 ,,	6·2	7·4	9·9	10·7	11·3	11·8	10·5	9·1
275	Nov. 4 ,,	11·8	12·5	11·0	11·3	12·5	12·4	14·2	10·4
276	Dec. 3 ,,	9·9	13·2	13·1	8·9	9·0	10·9	9·7	9·9
277	Jan. 2, 1870	10·8	12·7	13·0	12·8	12·4	14·1	12·4	11·9
278	Jan. 31 ,,	12·3	11·9	9·9	11·5	14·3	14·4	15·5	14·8
279	Mar. 2 ,,	12·2	11·3	10·7	9·8	10·4	10·7	10·3	10·8
(280)	April 1 ,,	10·7	11·2	11·0	11·1	10·4	9·4	10·0	10·4
(281)	April 30 ,,	9·3	7·7	8·8	9·9	8·9	8·8	9·0	8·1

No. of lunation.	Date of new moon beginning lunation.	(0).	(1).	(2).	(3).	(4).	(5).	(6).	(7).
(282)	May 30, 1870	8·8°	8·8°	8·3	8·4°	8·4°	6·2°	6·1°	7·3°
(283)	June 29 „	7·1	7·1	6·1	5·7	5·2	4·8	4·5	5·2
(284)	July 28 „	4·9	5·4	6·3	7·1	6·0	5·9	6·6	6·5
(285)	Aug. 27 „	5·9	6·0	5·9	7·7	8·0	7·6	6·0	5·0
286	Sept. 25 „	6·5	8·2	8·8	7·4	7·6	10·1	9·3	9·9
287	Oct. 24 „	9·1	8·1	9·8	9·6	8·5	10·9	12·7	12·9
288	Nov. 23 „	10·3	10·2	12·5	12·2	12·3	13·4	12·4	11·8
289	Dec. 22 „	12·1	11·0	10·4	12·4	10·9	12·4	11·4	12·4
290	Jan. 21, 1871	13·8	14·8	14·2	12·1	15·9	14·8	12·0	10·3
291	Feb. 19 „	12·0	11·8	11·0	10·4	11·6	11·3	13·0	13·8
(292)	Mar. 21 „	12·5	11·2	10·5	9·6	9·3	10·8	11·3	10·6
(293)	April 20 „	10·6	10·5	9·6	9·3	9·1	8·4	8·1	9·2
(294)	May 19 „	8·4	8·2	7·8	8·7	9·8	10·1	8·1	7·3
(295)	June 18 „	7·1	6·8	5·6	5·8	6·7	6·7	6·1	6·2
(296)	July 17 „	7·1	6·8	6·4	6·1	5·3	4·4	5·2	5·6
(297)	Aug. 16 „	5·9	6·3	6·6	6·6	6·1	6·6	6·6	6·1
298	Sept. 15 „	7·0	7·7	7·3	6·9	7·7	8·7	8·9	8·4
299	Oct. 14 „	9·8	11·2	11·1	11·4	12·5	14·6	14·0	12·4
300	Nov. 12 „	9·4	8·4	9·2	10·3	10·7	10·9	11·6	12·1
301	Dec. 12 „	12·3	11·3	11·6	11·3	13·0	12·0	12·9	15·0
302	Jan. 10, 1872	15·2	13·7	14·3	13·1	13·1	11·2	11·0	13·2
303	Feb. 9 „	15·1	15·2	15·2	13·8	15·2	11·8	11·4	11·8
(304)	Mar. 9 „	10·3	9·8	10·6	12·2	12·1	10·2	11·3	11·0
(305)	April 8 „	9·9	10·0	10·4	10·7	9·7	9·8	11·2	9·7
(306)	May 7 „	8·3	9·3	9·3	9·0	8·5	8·0	8·2	7·9
(307)	June 6 „	7·5	9·1	8·8	6·9	6·8	6·2	5·8	5·4
(308)	July 5 „	5·7	6·0	5·7	4·8	4·3	5·1	6·1	7·0
(309)	Aug. 4 „	6·6	5·5	5·5	6·2	6·3	6·6	6·6	5·9
(310)	Sept. 3 „	6·5	7·6	8·5	7·7	5·7	6·7	7·6	7·6
311	Oct. 2 „	8·0	7·7	9·3	11·9	10·9	9·9	11·0	13·2
312	Nov. 1 „	13·5	13·6	13·3	13·7	14·6	15·3	12·8	11·4
313	Nov. 30 „	10·3	8·2	9·5	9·4	10·4	11·9	13·1	13·4
314	Dec. 30 „	14·5	13·4	13·1	16·6	17·2	13·0	12·3	12·2
315	Jan. 28, 1873	11·7	12·6	12·4	10·5	11·1	9·8	10·8	12·2
316	Feb. 27 „	11·7	10·5	10·5	10·5	10·1	10·7	10·6	10·8
(317)	Mar. 28 „	10·7	9·2	10·5	9·0	8·3	9·4	10·1	9·7
(318)	April 27 „	8·7	8·5	9·0	8·9	8·0	7·8	8·1	8·8
(319)	May 26 „	9·8	8·7	5·6	6·4	6·2	6·1	6·6	6·8
(320)	June 25 „	7·9	6·9	5·3	5·2	4·2	4·2	4·3	5·1
(321)	July 24 „	5·6	5·0	4·3	5·5	5·4	6·4	6·9	7·4
(322)	Aug. 23 „	5·3	4·7	4·7	5·5	5·9	6·8	7·5	7·7
323	Sept. 21 „	7·7	7·3	8·0	8·4	10·7	11·8	10·8	10·3
324	Oct. 21 „	11·2	11·2	12·9	13·4	11·7	9·7	10·9	12·5
325	Nov. 20 „	12·5	13·4	14·6	14·9	15·2	13·2	12·2	14·7
326	Dec. 20 „	14·7	13·2	13·6	13·4	13·5	13·4	14·5	15·0
327	Jan. 18, 1874	14·5	12·8	11·8	11·1	12·6	12·8	11·4	14·0
328	Feb. 17 „	15·8	12·6	12·4	14·4	12·5	10·6	12·2	12·5
(329)	Mar. 18 „	10·8	9·9	10·8	12·1	10·9	10·5	9·1	10·1
(330)	April 16 „	10·1	10·2	10·2	9·4	9·6	8·4	7·9	8·4
(331)	May 16 „	7·7	8·1	8·8	8·6	9·7	10·3	8·1	7·4
(332)	June 14 „	6·9	5·8	4·9	5·4	7·3	7·4	6·9	5·6
(333)	July 13 „	5·0	4·0	3·3	4·8	5·3	3·8	5·5	6·5
(334)	Aug. 12 „	6·0	5·8	6·1	7·0	8·0	8·2	7·3	6·4
335	Sept. 10 „	6·1	6·2	5·7	6·3	7·4	8·4	8·3	8·6
336	Oct. 10 „	10·2	10·1	10·8	11·3	11·4	11·4	11·8	10·9
337	Nov. 9 „	12·2	14·2	13·2	11·9	12·6	12·1	11·5	12·8
338	Dec. 9 „	11·9	12·7	11·2	13·8	15·3	13·2	12·7	14·8

No. of lunation.	Date of new moon beginning lunation.	(0).	(1).	(2).	(3).	(4).	(5).	(6).	(7).
339	Jan. 7, 1875	14·7°	12·3°	11·6°	11·9°	12·7°	13·2°	13·9°	13·3°
340	Feb. 6 ,,	12·3	12·7	11·7	11·3	10·6	10·5	11·8	12·2
(341)	Mar. 8 ,,	12·4	12·2	11·9	10·4	10·3	9·8	8·7	9·6
(342)	April 6 ,,	10·1	10·7	10·4	10·3	10·5	9·5	8·7	8·7
(343)	May 5 ,,	8·6	8·8	9·4	9·6	9·5	8·6	8·0	7·5
(344)	June 4 ,,	8·1	8·9	8·3	7·0	6·4	6·9	6·5	6·2
(345)	July 3 ,,	7·2	7·1	6·7	6·0	6·6	6·3	6·6	6·7
(346)	Aug. 1 ,,	6·5	5·6	4·9	5·8	6·6	7·0	7·3	7·2
(347)	Aug. 31 ,,	6·1	4·5	4·9	7·3	6·2	6·1	6·8	7·1
348	Sept. 29 ,,	8·0	9·4	9·2	11·1	12·7	12·4	10·6	8·1
349	Oct. 29 ,,	8·6	10·6	13·3	12·7	11·7	11·5	12·0	14·3
350	Nov. 28 ,,	13·7	11·9	11·8	12·0	11·8	12·5	13·3	13·1
351	Dec. 28 ,,	13·4	13·6	13·7	13·3	13·4	12·8	11·5	12·3
352	Jan. 26, 1876	13·6	12·0	12·5	14·2	14·1	12·9	12·4	12·9
353	Feb. 25 ,,	12·7	12·1	10·1	9·8	10·7	10·3	10·3	11·3
(354)	Mar. 26 ,,	11·4	10·1	9·5	9·8	9·2	9·3	11·0	10·8
(355)	April 24 ,,	9·9	8·9	8·7	9·4	9·1	8·9	8·9	8·5
(356)	May 23 ,,	8·6	8·3	9·0	8·8	8·6	7·8	5·7	6·4
(357)	June 22 ,,	7·7	7·1	7·3	7·0	6·4	6·4	5·2	5·0
(358)	July 21 ,,	6·3	5·6	5·1	6·8	6·8	6·6	7·1	7·1
(359)	Aug. 19 ,,	6·5	6·4	6·3	5·5	6·8	5·9	8·7	8·2
360	Sept. 18 ,,	7·2	7·8	8·7	9·2	10·4	11·2	10·4	9·9
361	Oct. 17 ,,	10·8	11·8	12·7	13·2	12·8	12·3	11·5	10·9
362	Nov. 16 ,,	11·3	11·6	13·6	14·3	13·0	12·4	12·9	13·4
363	Dec. 15 ,,	13·2	13·6	15·3	15·2	15·4	15·6	16·0	12·4
364	Jan. 14, 1877	9·9	11·0	11·0	11·1	11·9	11·8	9·7	11·9
365	Feb. 13 ,,	13·8	12·4	12·3	12·6	11·5	12·1	12·5	11·3
(366)	Mar. 15 ,,	12·2	12·1	11·2	10·5	10·4	12·3	11·7	10·7
(367)	April 13 ,,	9·8	10·4	10·0	9·5	9·9	10·6	10·1	9·2
(368)	May 13 ,,	8·4	8·7	8·7	9·0	9·4	9·0	8·6	9·6
(369)	June 11 ,,	9·5	10·0	8·0	5·6	6·8	6·9	7·7	7·9
(370)	July 11 ,,	6·7	6·5	6·6	7·2	7·8	7·6	7·1	7·3
(371)	Aug. 9 ,,	7·6	8·6	8·0	7·5	6·9	5·7	6·8	7·5
(372)	Sept. 7 ,,	7·7	8·2	8·6	8·3	8·5	9·2	8·8	6·9
373	Oct. 7 ,,	6·9	8·4	8·2	8·9	9·2	9·9	11·4	12·4
374	Nov. 5 ,,	12·0	12·5	12·9	12·7	12·3	11·3	12·9	13·4
375	Dec. 5 ,,	11·0	10·1	11·7	10·0	9·9	11·3	10·8	12·1
376	Jan. 3, 1878	14·5	12·4	11·3	12·4	13·1	13·0	11·2	12·5
377	Feb. 2 ,,	14·1	11·9	14·4	14·2	15·0	14·3	13·0	12·3
378	Mar. 4 ,,	11·9	11·9	11·5	11·0	11·2	11·4	11·2	10·7
(379)	April 3 ,,	10·8	11·5	10·5	11·1	12·3	10·8	9·7	9·8
(380)	May 2 ,,	9·9	9·2	9·3	10·1	9·6	9·3	9·1	8·9
(381)	June 1 ,,	8·7	8·8	9·2	8·8	9·8	8·4	8·6	6·9
(382)	June 30 ,,	4·5	5·6	7·0	6·8	7·5	7·9	7·2	6·1
(383)	July 30 ,,	5·8	6·6	6·0	5·7	4·8	6·0	6·4	7·2
(384)	Aug. 28 ,,	7·8	8·4	7·7	6·5	7·6	7·8	6·3	6·9
385	Sept. 26 ,,	8·0	10·1	10·3	9·6	10·5	10·4	9·5	9·4
386	Oct. 26 ,,	11·1	11·2	9·6	9·9	10·5	10·5	11·4	11·6
387	Nov. 24 ,,	11·2	11·1	12·4	14·4	14·4	14·9	15·3	14·8
388	Dec. 24 ,,	13·1	12·4	14·2	14·5	12·1	11·1	13·0	14·4
389	Jan. 22, 1879	15·3	15·6	14·5	13·8	12·8	13·6	12·5	11·7
390	Feb. 21 ,,	12·2	11·8	11·0	13·5	14·1	12·2	12·0	13·9
(391)	Mar. 23 ,,	12·5	11·9	12·2	10·5	10·6	11·6	11·7	11·0
(392)	April 21 ,,	10·8	9·9	8·8	9·6	10·4	9·7	8·8	8·0
(393)	May 21 ,,	9·4	10·2	7·3	6·6	7·1	7·7	8·6	9·2
(394)	June 20 ,,	7·4	5·8	5·3	5·3	6·6	6·5	6·2	7·1
(395)	July 19 ,,	7·6	8·1	7·8	6·7	5·3	4·9	5·8	5·6

No. of lunation.	Date of new moon beginning lunation.	(0).	(1).	(2).	(3).	(4).	(5).	(6).	(7).
(396)	Aug. 18, 1879	6·3°	5·9°	6·8°	7·7°	6·8°	7·1°	8·3°	8·7°
397	Sept. 16 ,,	8·4	7·9	7·5	7·5	7·8	7·0	7·3	9·1
398	Oct. 15 ,,	10·5	12·2	12·8	13·0	12·3	10·1	12·8	16·0
399	Nov. 14 ,,	14·7	13·8	13·5	14·6	16·6	15·1	13·6	15·0
400	Dec. 13 ,,	15·1	13·5	15·6	15·4	13·1	14·2	14·7	14·2
401	Jan. 12, 1880	13·4	13·8	12·6	15·6	17·6	15·0	13·1	12·9
402	Feb. 10 ,,	14·4	12·1	12·1	13·2	14·2	14·8	12·5	11·8
(403)	Mar. 11 ,,	11·3	11·5	10·4	9·4	10·9	12·3	12·5	11·5
(404)	April 9 ,,	10·0	8·8	9·9	10·6	10·1	9·7	9·9	10·0
(405)	May 9 ,,	9·5	9·1	9·3	9·3	9·0	8·4	7·9	9·1
(406)	June 8 ,,	9·0	7·9	7·5	7·8	8·7	8·2	6·6	6·6
(407)	July 7 ,,	7·1	6·9	6·0	7·3	6·9	6·6	6·6	7·1
(408)	Aug. 6 ,,	6·5	6·2	6·5	6·8	8·0	8·4	7·9	7·3
(409)	Sept. 4 ,,	7·1	8·3	9·0	6·6	6·2	7·4	7·1	7·7

8. The whole series of 409 lunations gives the following result:—

Phase of lunation..................	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Value of range.....................	10·69	10·67	10·67	10·73	10·77	10·66	10·61	10·66	(A)

A series which, like that found by Dr. Stewart from the Kew temperature-ranges, has two maxima and two minima, but every turning-point in the Bombay series occurs somewhat later on in the lunation than the corresponding turning-point in the Kew series, and the range (0°·16) of the Bombay series is less than that (0°·46) of the Kew series. The sum of the four left-hand numbers (42·76) is also larger, as in the Kew series, than the sum (42·70) of the four right-hand numbers. Series (A) is curved in fig. 2.

Dividing the whole series into two parts, we obtain—

Phase of lunation..................	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Range (1847·75 to 1863·75) ...	11·32	11·36	11·42	11·53	11·49	11·32	11·30	11·25	(B)
,, (1863·75 to 1880·75) ...	10·10	10·01	9·96	9·98	10·10	10·05	9·96	10·11	(C)

Upon which it may be remarked that, though possessing, of necessity, points of similarity to the series (A), these two series are far from being identical with it or with each other.

D. *Semi-annual Lunar Variation.*

9. In dividing the lunations into winter and summer lunations, we had inadvertently chosen the 21st March and 23rd September, instead of the 31st March and 30th September, as the dates between which,

if the middle of a lunation occurred, the lunation should be considered a summer lunation, and the serial numbers of such lunations in Table III are enclosed in parentheses to distinguish them from the others, which are to be considered as those of winter lunations. The average date of new moon will be about the 22nd of each month, and, accordingly, in eliminating the residual effect of the annual variation upon the lunar variations for the winter and summer half-years, the beginnings of the several months have been taken to correspond with the average time of first quarter of the moon, and the respective half-years have been made to commence after the lapse of three-quarters of the months of September and March. The numbers at the foot of Table Ia having been curved on a large scale, the ordinates of the curve were measured for every eighth of a month, and the averages were taken of the six sets of eight numbers corresponding to the winter lunations, and of the six sets corresponding to the summer lunations. These were then multiplied by 1·069, the ratio of the average scale of the 409 lunations to the scale of the period 1873 to 1880 (that is, of Table Ia), and the variations were then taken—with the results that will now be made use of.

The values of temperature-range found for the eight phases of the winter lunations, of which there are 199, are—

Phase of lunation	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Mean range	12·50	12·54	12·73	12·89	13·00	12·92	12·91	13·02	(D)
Correction applicable to winter months	+ ·33	+ ·25	+ ·13	− ·01	− ·09	− ·15	− ·20	− ·24	(E)
Correct value of winter lunar range	12·83	12·79	12·86	12·88	12·91	12·77	12·71	12·78	(F)

and for the eight phases of the summer lunations, of which there are 210, they are—

Phase of lunation	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Mean range	8·98	8·89	8·72	8·69	8·66	8·52	8·42	8·43	(G)
Correction applicable to summer months	− ·30	− ·21	− ·12	− ·04	+ ·04	+ ·12	+ ·20	+ ·27	(H)
Correct value of summer lunar range	8·68	8·68	8·60	8·65	8·70	8·64	8·62	8·70	(I)

Fig. 3 represents the corrected variation for the summer months, and fig. 4 for the winter months. The winter curve, like that of Kew, is mainly a single period curve, and its maximum and minimum phases both occur somewhat later than at Kew. The summer curve is of smaller range; again, like the corresponding curve at Kew, but unlike the latter, it is a very regular double period curve.

10. The excesses of the two series (F) and (I) above the series (A), that is, the semi-annual inequalities of the lunar variations, are—

Phase of lunation	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Winter inequality	+2·14	+2·12	+2·19	+2·15	+2·14	+2·11	+2·10	+2·12	(J)
Summer inequality	−2·01	−1·99	−2·07	−2·08	−2·07	−2·02	−1·99	−1·96	(K)

Curves representing the series (K) and (J) form figs. 7 and 8 respectively, and they are, necessarily, nearly opposite to each other in character, and, like those of Kew, they are in the main single period waves, but with the maximum and minimum phases occurring later at Bombay than at Kew.

11. The next two series (L) and (M) are the winter lunar variations for the periods 1847·75 to 1863·25 (sixteen winters), and 1863·75 to 1880·25 (seventeen winters) obtained in the same manner as series (F). They are curved in figs. 5 and 6, which though possessing little likeness to each other, have, of course, each points of similarity with fig. 4.

Phase of lunation		(0)	(1)	(2)	(3)	(4)	(5)	(6	(7)	
Winter lunar variation.	First 16 years	13·43	13·58	13·69	13·79	13·62	13·43	13·46	13·36	(L)
	Last 17 years	12·26	12·03	12·06	12·01	12·23	12·15	12·00	12·23	(M)

It may be noted that the series (L) and (M) bear nearly the same relation to the series (F) that the series (B) and (C) respectively bear to the series (A), a fact which, combined with the knowledge that the summer lunar variation is of small extent, implies that the winter months contribute nearly the whole of the irregularity which distinguishes one half of the period of thirty-three years from the other.

E. *Possible Variation of the Lunar Effect with the Sun-spot Period.*

12. In order to examine the relation of the winter lunar variation to the sun-spot period, the winters chosen as corresponding to the minimum and maximum respectively, of solar activity, are 1854–55 to 1856–57, 1861–62 to 1866–67, 1872–73 to 1874–75, and 1857–58 to 1860–61, 1867–68 to 1871–72. These groups of winters give results as follows:—

Phase of lunation	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Winter variation (minimum period) corrected for (E)	12·90	12·67	12·49	12·35	12·54	12·62	12·71	12·89	
Deduct (F)	12·83	12·79	12·86	12·88	12·91	12·77	12·71	12·78	
Supposed effect of solar minimum	+ ·07	− ·12	− ·37	− ·53	− ·37	− ·15	·00	+ ·11	(N)

and

Phase of lunation	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Winter variation (maximum period) corrected for (E)......	12·14	12·35	12·51	12·31	12·44	12·22	12·04	12·15	
Deduct (F)...........................	12·83	12·79	12·86	12·88	12·91	12·77	12·71	12·78	
Supposed effect of solar maximum	−·69	−·44	−·35	−·57	−·47	−·55	−·67	−·63	(P)

13. For the sake of comparability with Dr. Stewart's results for Kew, which may possibly refer to winters a year later in every case than those named above, we have repeated these calculations on that supposition, and have obtained, in lieu of series (N) and (P) the following:—

Phase of lunation	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Supposed effect of solar minimum	−·10	−·21	−·34	−·45	−·27	−·25	−·12	·00	(N′)
Supposed effect of solar maximum	−·58	−·44	−·41	−·63	−·53	−·38	−·52	−·51	(P′)

The series (N), (N′), (P), (P′) are curved in figs. 9 to 12. Contrary to the Kew series, they show that the temperature-range is somewhat less when sun-spots are excessive than when they are defective, the mean values of the several series being −·17, −·21, −·54 and −·50 respectively. Figs. 9 and 10 are much alike, and imply that the general winter lunar variation found in this way is subject to a pronounced change of character during the time of deficient sun-spots, having superimposed upon it a variation of single period and of greater range than its own. This affords a partial explanation of the great difference between the curves of winter lunar variations for the first sixteen and last seventeen years (figs. 5 and 6), the latter period being made up of years of deficient sun-spots in greater degree than the former period.

"Sun-spots and Terrestrial Phenomena. II. On the Variations of the Daily Range of the Magnetic Declination, as recorded at the Colaba Observatory, Bombay." By CHARLES CHAMBERS, F.R.S., Superintendent. Received May 30. Read June 15, 1882.

The present, like the preceding, investigation is on the model of one by Dr. Balfour Stewart, dealing with similar records obtained at the

Kew Observatory.* The records extend, in the present case, from June 1, 1847, to December 31, 1872, and consist of differences (always taken to be of positive sign) between the highest and the lowest values of easterly declination observed by Grubb's declination magnetometer† on every observation day of the period. Until the end of the year 1865 the observation day was the Göttingen astronomical day; after that time it was the Bombay civil day. The daily differences were obtained from hourly observations made on all days except Sundays and a few holidays in each year. Grubb's declination magnetometer is of the well known form described in the Report of the Committee of Physics of the Royal Society, 1840 (p. 13). Up to 1868·00 each individual entry in the register of the scale-reading of the instrument was at once converted into easterly declination in minutes, and the daily ranges are the differences of such converted readings, but after the date named the differences of the scale-readings were first taken, and then converted into minutes. The ranges *include* the effect of disturbance.

A. *Annual Variation of Declination-Range.*

2. The year being divided into forty-eight equal parts, commencing with the midnight between the 31st December and 1st January,‡ means were taken of the ranges for the fifteen days preceding and fifteen days following the nearest midnight to the commencement of each 48th part of the year. Attributing four of the 48ths of a year to each month, and designating as "monthly means" the thirty-day means thus obtained, the following table exhibits each of the forty-eight results for each year, and on the average of all the years:—(See pp. 250 and 251).

The numbers in the last column are taken to represent the annual variation—combined with the annual mean value—of declination-range.

B. *Variation of Long Period.*

3. Proceeding now on Dr. Stewart's hypothesis as to the relation between declination-range and solar activity, we divide the numbers in each line of Table I by the mean number (in the last column) of that line, and multiply the quotient by 1000, thus obtaining a table "exhibiting the monthly means of declination-range (forty-eight points to each year), the mean value of the range for the whole series

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* "Proc. Roy. Soc.," vol. 26, p. 102.

† On the rare occasions when this instrument was under adjustment, the blanks in its register were filled up from the register of a small declination magnetometer which was used as a subsidiary instrument.

‡ Leap-years were taken to contain 366 days, and other years 365 days.

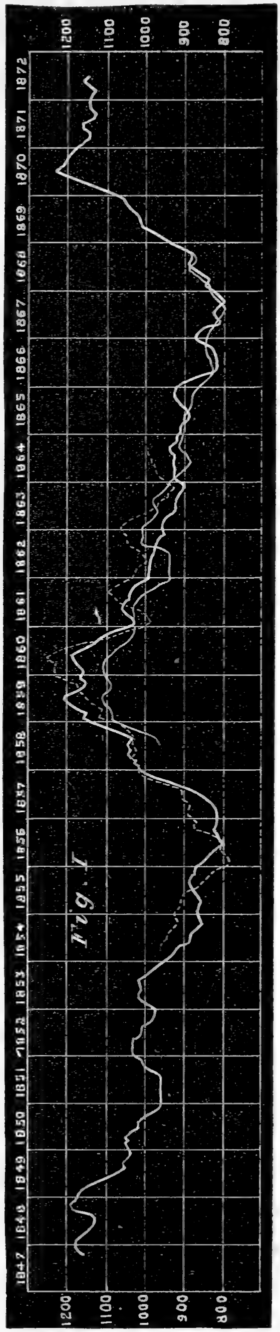

Fig. 1

1200
1100
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1872

Table I.—Containing Monthly Means (48 to the year) of the Diurnal which the Middle Date is the very commencement of the Year,

		1847.	1848.	1849.	1850.	1851.	1852.	1853.	1854.	1855.	1856.	1857.	1858.
January	(0)......	...	4·22	3·20	2·62	3·04	2·56	2·80	3·05	2·45	2·85	2·44	3·37
,,	(1)......	...	3·50	3·54	2·93	3·30	3·15	2·87	3·22	2·51	2·99	2·58	3·28
,,	(2)......	...	3·72	3·81	3·11	3·29	3·11	2·86	3·27	2·50	3·02	2·49	3·38
,,	(3)......	...	3·65	3·85	3·91	3·34	3·19	2·79	2·96	2·53	2·80	2·43	3·01
February	(0)......	...	3·37	3·75	3·85	3·07	3·26	2·70	2·91	2·49	2·58	2·45	2·65
,,	(1)......	...	3·45	3·65	3·56	2·81	3·45	2·65	2·70	2·47	2·30	2·43	2·74
,,	(2)......	...	3·20	3·77	3·33	2·50	3·31	2·47	2·72	2·41	2·24	2·39	2·74
,,	(3)......	...	3·25	3·76	2·55	2·47	3·51	2·47	2·67	2·47	2·17	2·22	2·99
March	(0)......	...	3·67	3·86	2·80	2·65	3·52	2·66	2·58	2·64	2·18	2·04	3·58
,,	(1)......	...	3·96	3·98	3·11	2·86	3·31	2·95	2·68	2·61	2·36	2·19	3·85
,,	(2)......	...	4·42	4·31	3·57	3·19	3·63	3·27	3·04	2·96	2·56	2·29	4·06
,,	(3)......	...	4·69	5·10	4·01	3·55	4·06	3·87	3·44	3·45	3·35	2·56	4·53
April	(0)......	...	4·66	5·52	4·39	3·92	4·37	4·57	3·87	3·57	3·91	2·91	4·69
,,	(1)......	...	4·66	5·70	4·65	3·99	4·80	4·71	4·39	4·27	4·21	3·26	4·78
,,	(2)......	...	4·91	5·45	4·67	4·08	5·41	4·92	4·37	4·40	4·17	3·67	5·03
,,	(3)......	...	5·28	5·15	5·11	4·34	5·43	4·86	4·61	4·60	3·91	4·10	4·97
May	(0)......	...	5·49	5·13	5·49	4·41	5·37	4·56	4·84	4·82	3·57	4·58	5·07
,,	(1)......	...	5·45	5·57	5·56	4·42	5·57	4·93	4·86	4·69	3·60	5·02	5·36
,,	(2)......	...	5·45	5·75	5·94	4·82	5·33	4·96	5·09	4·51	3·72	5·03	5·49
,,	(3)......	...	5·45	6·11	5·79	5·00	5·35	5·13	5·11	4·58	3·80	4·88	5·42
June	(0)......	...	5·39	6·13	5·67	5·11	5·51	5·65	4·99	4·41	3·90	4·73	5·12
,,	(1)......	...	5·52	5·84	5·75	5·45	5·48	5·57	4·90	4·39	3·98	4·18	4·85
,,	(2)......	5·22	5·46	5·78	5·78	5·72	5·42	5·86	4·78	4·60	4·20	4·23	5·02
,,	(3)......	5·46	5·55	5·53	5·81	5·75	5·39	5·94	4·89	4·65	4·04	4·34	5·16
July	(0)......	5·19	5·92	5·80	5·97	6·00	5·20	5·78	4·86	4·42	4·06	4·37	5·33
,,	(1)......	5·34	6·24	5·78	5·81	5·85	5·40	5·89	4·62	4·42	4·26	4·52	5·42
,,	(2)......	5·01	6·40	5·89	5·65	5·45	5·38	5·82	4·57	4·21	4·30	4·26	5·43
,,	(3)......	4·85	6·68	6·10	5·52	5·10	5·37	5·95	4·44	4·12	4·53	4·30	5·44
August	(0)......	4·72	6·62	5·90	5·20	4·75	5·74	5·97	4·43	4·23	4·90	4·30	5·84
,,	(1)......	4·97	6·71	6·11	5·37	4·81	5·86	6·24	4·74	4·31	4·97	4·58	6·16
,,	(2)......	5·62	6·88	6·21	5·43	5·10	5·95	6·44	5·02	4·53	5·00	4·93	6·21
,,	(3)......	6·07	7·01	6·26	5·91	5·60	6·18	6·91	5·18	4·88	5·47	5·32	6·32
September	(0)......	6·64	7·21	6·37	6·10	5·58	5·98	7·09	5·22	5·62	5·51	5·70	6·22
,,	(1)......	6·55	6·86	6·16	6·40	5·88	5·14	6·88	5·15	5·91	5·49	6·06	6·06
,,	(2)......	6·54	6·52	6·09	6·18	5·63	4·82	6·42	5·05	5·48	5·25	6·12	6·02
,,	(3)......	6·19	6·00	5·47	5·74	5·42	4·19	5·38	4·61	5·13	4·40	5·67	5·59
October	(0)......	5·44	5·49	4·89	4·90	5·00	3·57	4·34	4·02	4·16	3·71	4·87	5·02
,,	(1)......	5·07	4·98	4·13	4·05	4·61	3·55	3·41	3·74	3·59	3·09	3·95	4·34
,,	(2)......	4·33	4·11	3·29	3·48	3·54	3·27	2·75	2·85	3·09	2·52	3·14	3·71
,,	(3)......	3·71	3·34	2·66	2·78	2·87	2·96	2·64	2·41	2·74	2·36	2·28	3·26
November	(0)......	3·32	2·58	2·50	2·56	2·63	2·84	2·54	2·14	2·55	2·05	2·18	3·03
,,	(1)......	3·04	2·76	2·37	2·37	2·52	2·54	2·48	2·04	2·38	1·89	2·30	2·98
,,	(2)......	2·95	2·89	2·80	2·18	2·39	2·52	2·36	2·16	2·39	2·00	2·30	2·55
,,	(3)......	2·88	3·11	2·93	2·24	2·50	2·74	2·44	2·34	2·33	2·23	2·50	2·48
December	(0)......	2·91	3·17	2·84	2·31	2·42	3·15	2·52	2·45	2·49	2·39	2·82	2·39
,,	(1)......	3·77	2·83	2·79	2·38	2·27	3·10	2·55	2·40	2·40	2·40	2·76	2·32
,,	(2)......	3·69	2·76	2·39	2·72	2·38	3·17	2·63	2·35	2·47	2·38	2·98	2·68
,,	(3)......	3·85	2·93	2·49	2·76	2·41	3·09	2·80	2·26	2·81	2·50	3·24	2·93

Declination-Range, thus:—January (0) gives the Monthly Mean of January (1) that for one Week after the commencement, and so on.

1859.	1860.	1861.	1862.	1863.	1864.	1865.	1866.	1867.	1868.	1869.	1870.	1871.	1872.	Mean.
3·17	3·08	2·65	2·79	2·91	2·14	3·00	2·71	2·38	2·00	2·32	3·23	3·15	2·92	2·84
3·42	3·09	2·90	2·70	2·96	2·41	3·15	2·95	2·90	1·82	2·69	2·96	3·28	2·99	2·96
3·60	3·18	3·05	2·89	3·02	2·53	3·34	2·88	2·84	1·90	2·94	2·95	3·32	3·08	3·04
3·39	3·19	3·05	3·19	3·02	2·66	3·24	2·93	2·79	2·13	2·97	2·86	3·21	3·89	3·08
3·44	3·20	3·02	3·29	2·85	2·82	2·95	2·90	2·75	2·09	3·03	3·21	3·24	3·97	3·03
3·30	3·19	2·86	3·05	2·72	2·66	2·94	2·98	2·29	2·21	2·69	3·36	3·07	3·90	2·94
3·12	3·04	2·97	2·97	2·65	2·69	2·69	3·10	2·06	2·17	2·48	3·40	3·18	3·96	2·86
3·15	3·26	2·95	2·53	2·60	2·69	2·50	2·83	2·13	2·04	2·39	3·36	3·32	3·42	2·79
3·57	3·36	3·26	2·51	2·65	2·87	2·53	2·87	2·13	2·30	2·60	3·12	3·40	3·62	2·92
3·81	3·61	3·54	2·99	3·07	3·17	2·83	2·92	2·20	2·52	2·84	3·48	3·90	4·25	3·16
4·33	4·35	3·90	3·72	3·17	3·28	3·13	3·13	2·84	2·85	3·35	3·93	4·43	4·66	3·53
4·83	4·63	4·58	4·30	3·31	3·84	3·35	3·56	3·07	3·45	3·86	4·75	4·73	4·41	3·97
5·37	4·96	4·72	4·84	3·79	4·10	3·62	4·08	3·44	3·81	4·39	5·31	5·18	4·68	4·35
6·14	5·12	4·80	4·88	4·11	4·23	3·86	4·12	3·97	4·60	4·75	5·57	5·76	4·98	4·65
6·29	5·15	4·84	4·75	4·23	4·44	4·30	3·93	4·12	5·09	4·57	5·66	5·98	5·41	4·79
7·00	5·46	4·77	4·66	4·82	4·33	4·67	3·96	4·43	5·11	4·52	5·24	6·01	6·06	4·94
6·98	5·77	4·80	4·28	4·97	4·25	4·73	3·81	4·46	5·05	4·94	5·37	5·86	6·27	4·99
6·87	6·01	5·09	4·47	5·19	4·42	4·75	3·76	4·38	4·82	5·21	5·61	5·82	6·33	5·11
6·88	5·99	5·10	4·78	5·56	4·94	4·68	3·93	4·21	4·55	5·49	6·05	5·85	6·43	5·22
6·55	5·97	5·33	5·11	5·57	5·37	4·60	3·98	4·14	4·73	5·77	6·32	6·21	6·07	5·29
6·34	5·90	5·75	5·41	5·42	5·72	4·66	3·90	4·03	4·99	6·01	6·50	6·52	6·04	5·35
6·05	6·13	5·80	5·42	5·11	5·91	4·75	4·04	4·11	4·88	6·11	6·55	6·17	5·83	5·31
6·17	6·77	5·70	5·64	5·02	5·47	4·93	4·05	4·29	4·86	6·15	6·71	6·51	5·35	5·37
5·77	7·05	5·78	5·42	5·15	5·35	4·98	3·90	4·37	4·60	6·09	6·68	6·26	5·49	5·36
5·88	6·74	5·85	5·31	5·22	5·18	4·60	3·79	4·50	4·35	5·86	6·60	6·01	5·54	5·32
5·84	6·69	5·57	5·13	5·37	4·87	4·38	3·73	4·59	4·22	6·01	6·94	6·44	5·58	5·34
5·84	6·34	5·76	5·00	5·46	4·67	4·16	3·58	4·76	4·15	6·10	6·67	6·31	5·85	5·26
5·55	6·50	5·98	5·10	5·23	4·63	4·45	3·57	4·62	4·06	6·09	6·96	6·09	6·16	5·28
5·31	7·52	6·05	5·47	5·31	4·87	4·71	3·62	4·49	4·14	6·33	7·03	6·55	6·28	5·40
5·65	7·99	6·62	5·84	5·05	5·23	4·95	3·71	4·62	4·68	5·85	7·19	6·82	6·23	5·59
6·48	8·47	7·04	5·80	5·12	5·82	5·01	4·13	4·50	5·25	5·92	7·62	7·13	6·46	5·85
8·81	8·36	7·15	6·08	5·18	6·06	4·76	4·39	4·70	5·90	5·93	7·70	7·45	6·46	6·16
9·00	7·67	7·11	6·01	5·19	6·18	4·77	4·43	4·72	6·36	6·25	7·60	7·44	6·61	6·25
8·82	7·12	7·05	5·82	4·96	5·92	4·82	4·59	4·47	5·83	6·35	7·64	6·87	7·07	6·15
6·84	6·63	6·41	5·40	4·62	5·45	4·82	4·09	4·16	5·31	5·89	7·45	6·38	6·95	5·79
5·56	6·14	5·62	5·20	4·29	4·61	4·50	4·15	3·80	4·94	5·50	6·86	5·82	6·45	5·28
5·06	5·82	4·90	4·60	3·67	3·82	4·25	4·01	3·36	4·16	4·30	6·27	5·04	5·98	4·64
4·65	5·02	3·83	4·13	3·46	3·17	3·84	3·75	3·19	3·70	3·59	5·37	4·47	5·13	4·07
4·44	4·02	3·29	3·68	2·98	2·87	3·33	3·56	3·03	3·18	3·21	4·55	3·94	4·25	3·48
4·00	3·34	2·93	2·85	2·67	2·63	3·24	3·14	2·63	2·45	2·71	4·22	3·53	3·62	3·00
3·37	2·78	2·69	2·62	2·53	2·40	3·13	2·82	2·57	2·18	2·53	3·83	3·17	3·15	2·72
2·89	2·52	2·60	2·34	2·49	2·12	2·87	2·51	2·21	2·11	2·53	3·43	3·00	2·74	2·54
2·66	2·35	2·40	2·30	2·58	2·03	2·71	2·51	2·33	2·14	2·40	3·11	2·83	2·66	2·48
2·73	2·21	2·34	2·41	2·65	2·13	2·39	2·59	2·39	2·31	2·45	2·76	2·50	2·75	2·51
3·24	2·37	2·53	2·43	2·62	2·26	2·21	2·62	2·35	2·45	2·57	2·48	2·48	2·72	2·58
3·43	2·42	2·81	2·51	2·45	2·54	2·27	2·49	2·41	2·29	2·70	2·46	2·47	2·94	2·62
3·50	2·45	2·89	2·64	2·27	2·56	2·41	2·36	2·09	2·17	2·91	2·51	2·47	3·03	2·65
3·52	2·64	2·89	2·67	2·15	2·95	2·59	2·26	2·00	2·34	3·32	2·7	2·76	...	2·75

for each point being reckoned =1000," which table contains in all 1,225 numbers. A second table is formed from this by taking the mean of twelve successive numbers and moving onward a step (*i.e.*, by one number) after each operation: a third table is formed from the second by taking means of pairs of successive numbers: the third table contains 1,213 entries, or six less at the beginning and six less at the end than the first table: the numbers in the third table—called "three-monthly values"—will be made use of further on, in the inquiry into planetary variations. Next all the numbers in the third table, except those opposite to the divisions (0) of the several months, being rejected, the means are taken of sets of three of the 304 remaining numbers, selected in the following manner, viz.:—the means of the 1st, 4th, and 7th entries, of the 2nd, 5th, and 8th entries, and so on, the result being placed opposite the middle number of the three in each case. Finally, means are taken of pairs of the successive numbers thus found, the final means being called "nine-monthly values," and corresponding in time to the division (2) of the several months. These final means are shown in Table II, in which the entries are reduced to 297.

Table II.—Declination-Range, Nine-monthly Values.

Year.	January (2).	February (2).	March (2).	April (2).	May (2).	June (2).	July (2).	August (2).	September (2).	October (2).	November (2).	December (2).
1847	...	...	...	...	...	...	...	...	...	...	1153	1168
1848	1176	1170	1166	1160	1144	1130	1123	1122	1134	1160	1182	1181
1849	1171	1165	1161	1155	1141	1118	1089	1061	1052	1053	1041	1035
1850	1036	1043	1050	1047	1048	1038	1018	1017	1017	1003	981	963
1851	956	957	959	958	958	957	957	963	978	1000	1006	1013
1852	1028	1031	1031	1034	1027	1015	1010	1010	1001	984	977	973
1853	974	995	1016	1020	1013	1008	1013	1019	1022	1015	997	976
1854	957	945	938	927	910	892	884	883	873	862	857	860
1855	865	863	859	855	857	866	876	887	891	882	878	878
1856	869	853	842	836	822	806	809	817	821	824	819	815
1857	815	815	819	823	833	844	863	905	935	953	983	1010
1858	1025	1025	1036	1047	1041	1044	1043	1033	1039	1056	1083	1114
1859	1141	1153	1151	1169	1193	1200	1207	1202	1179	1159	1159	1167
1860	1161	1157	1172	1181	1187	1189	1175	1152	1134	1122	1109	1079
1861	1047	1028	1027	1045	1062	1065	1060	1050	1048	1045	1039	1025
1862	1003	993	992	990	988	984	980	977	976	975	965	957
1863	959	961	960	953	937	926	928	927	918	911	906	900
1864	908	927	934	932	933	933	927	929	943	942	927	922
1865	918	914	916	910	896	892	898	905	914	922	929	926
1866	919	906	872	838	821	818	821	821	825	829	834	850
1867	864	867	856	833	812	807	819	822	806	797	801	813
1868	830	843	842	839	854	874	888	892	887	883	891	910
1869	929	949	977	998	1009	1014	1020	1033	1042	1047	1051	1054
1870	1065	1094	1130	1163	1189	1219	1230	1216	1207	1204	1200	1191
1871	1181	1169	1152	1151	1162	1170	1164	1140	1134	1145	1148	1147
1872	1146	1140	1135	1140	1160	1166	1148					

The numbers in the table are curved (in a strong line) in fig. 1, and the comparable numbers obtained by Dr. Stewart* for Kew and

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* "Proc. Roy. Soc.," vol. 26, p. 109, and vol. 28, p. 84.

Trevandrum are curved on the same form—the former in a weaker and the latter in an interrupted line.

4. On these curves we may remark that they present such a general correspondence of movement, and approach to simultaneity, that any conclusions as to the relations of the declination-range to solar activity that may be drawn in respect of one of them will apply generally in respect of the others also. The sun-spot period is distinctly followed by them all — three showing the maximum of 1859–60, and two the minima about 1856 and 1866–67. The general correspondence of the curves will perhaps be most readily apprehended by noting the most marked cases of departure from it: these are—(1) that the elevation, in the middle of 1859, in the Kew and Bombay curves, has no counterpart in that of Trevandrum, but only a slight inflection of a continued rise; (2) that the depression, at the beginning of 1861, at Trevandrum and Bombay, is all but absent at Kew; and (3) that the elevation, near the end of 1862, at Kew and Trevandrum, has no counterpart at Bombay.

Features of the Trevandrum and Bombay curves that are perhaps worth noting are that the great rise from 1856 to 1860 begins earlier and ends later at Trevandrum than at Bombay, that it begins lower and ends higher, and that the turnings at beginning and end are sharper in the same case.

C. *Lunar Annual Variation.*

D. *Semi-annual Lunar Variation.*

5. The processes by which the lunar variations of declination-range have been brought out were the same as were applied to the temperature-ranges in paragraphs 7 to 13 of the preceding investigation; but the observations made use of in the case of the declination-ranges are those for the twenty-five winters and twenty-five summers embraced between 1847·75 and 1871·75. In this case a lunation was taken to be a winter one if the middle of it occurred between the 1st October and 1st April, and the remaining lunations were taken as summer ones; and—in correspondence with this—the elimination of the part of the winter or summer lunar variation due to the annual variation of the declination-range was effected by the same division of the year in respect of summer and winter. The following table shows the mean values of the declination-range for each of the eight phases of each of the 309 lunations of the period, and the summer lunations are distinguished in it by having their serial numbers enclosed by parentheses.

Table III.—Exhibiting the Declination-Ranges grouped according to Lunations.

Running No.	Lunation commencing new moon.	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1	October 8, 1847	5·439	4·126	3·542	3·617	4·937	4·029	3·005	2·850
2	November 7, ,,	2·553	2·439	2·352	2·393	3·651	4·309	2·758	2·215
3	December 7, ,,	2·587	2·774	3·285	7·905	7·108	2·747	2·719	3·331
4	January 6, 1848	3·187	3·613	3·989	4·656	4·046	3·594	3·519	2·850
5	February 4, ,,	3·011	2·630	2·621	3·441	4·544	4·744	2·576	2·702
6	March 5, ,,	3·228	3·359	4·183	4·498	5·218	5·407	4·714	4·133
(7)	April 3, ,,	4·171	4·203	4·326	4·646	5·592	5·248	5·316	5·654
(8)	May 2, ,,	5·482	5·706	5·774	5·242	5·642	5·087	5·051	5·528
(9)	June 1, ,,	6·134	5·453	5·037	4·986	4·905	5·900	5·614	5·446
(10)	,, 30, ,,	6·009	5·866	6·004	6·118	6·542	7·096	6·616	6·296
(11)	July 29, ,,	6·519	7·376	6·195	5·941	7·342	8·571	6·822	6·840
(12)	August 28, ,,	7·440	6·908	7·171	6·772	7·079	7·307	5·758	5·454
13	September 26, ,,	6·022	5·687	5·242	4·409	4·957	4·945	3·989	3·257
14	October 26, ,,	3·211	2·398	1·940	1·827	1·907	2·343	4·029	3·639
15	November 25, ,,	3·457	3·102	2·589	2·867	2·787	2·294	2·575	3·022
16	December 25, ,,	2·521	2·913	3·348	3·490	3·194	2·940	4·298	4·413
17	January 23, 1849	4·304	4·052	3·732	3·619	3·422	3·104	2·953	3·890
18	February 22, ,,	4·628	4·512	3·966	3·639	3·570	3·245	3·663	4·578
(19)	March 24, ,,	4·985	5·488	6·296	6·908	6·370	5·402	5·053	5·150
(20)	April 22, ,,	5·322	4·951	4·635	5·034	5·569	5·957	5·116	5·096
(21)	May 21, ,,	7·034	6·833	5·417	5·372	7·148	6·908	5·093	5·417
(22)	June 20, ,,	6·042	5·775	5·186	5·262	6·136	5·797	6·135	6·099
(23)	July 19, ,,	6·249	5·562	5·276	5·563	6·319	6·496	5·574	5·671
(24)	August 17, ,,	5·986	6·827	6·078	6·127	6·484	6·854	6·061	5·701
(25)	September 16, ,,	5·564	6·256	6·307	5·329	5·150	4·368	3·411	3·474
26	October 15, ,,	3·153	3·090	2·964	2·605	2·468	1·871	1·752	2·678
27	November 14, ,,	2·816	2·486	2·483	4·161	4·268	2·567	2·236	2·420
28	December 14, ,,	2·328	2·357	2·419	2·823	2·706	2·454	2·534	2·483
29	January 13, 1850	3·284	3·598	3·559	3·757	3·519	3·282	5·699	4·921
30	February 11, ,,	2·758	2·288	2·437	3·179	2·506	2·182	2·494	3·066
31	March 13, ,,	3·439	4·127	4·006	3·941	4·326	3·639	4·142	5·151
(32)	April 12, ,,	5·265	4·635	4·407	4·865	4·578	4·710	5·413	6·066
(33)	May 11, ,,	6·994	6·547	5·013	5·162	5·746	5·851	5·894	5·449
(34)	June 9, ,,	5·472	6·071	5·803	5·665	5·757	5·975	5·826	5·576
(35)	July 9, ,,	6·558	6·261	6·124	5·442	4·887	5·563	5·608	4·725
(36)	August 7, ,,	4·888	5·501	5·442	4·715	5·574	5·940	6·089	6·427
(37)	September 5, ,,	6·839	6·964	6·249	6·593	6·249	6·032	5·230	4·732
38	October 5, ,,	4·892	3·948	3·410	2·964	2·952	3·117	2·906	2·663
39	November 3, ,,	2·413	1·944	2·018	2·328	2·231	1·648	2·059	2·458
40	December 3, ,,	2·437	2·025	2·174	2·857	2·770	2·643	3·392	3·605
41	January 1, 1851	2·691	2·842	3·536	3·694	3·336	3·462	3·783	3·530
42	,, 31, ,,	3·101	2·666	2·557	2·374	2·609	2·689	2·323	2·582
43	March 2, ,,	2·649	2·528	2·614	2·856	3·330	3·406	3·582	3·479
(44)	April 1, ,,	3·902	3·694	4·057	4·429	4·910	4·429	3·560	3·937
(45)	,, 30, ,,	4·143	4·543	5·242	4·910	4·898	4·120	3·754	5·207
(46)	May 30, ,,	6·146	6·085	5·528	5·273	5·287	5·356	4·887	5·322
(47)	June 28, ,,	7·348	7·406	5·860	5·493	5·298	5·986	4·395	5·082
(48)	July 28, ,,	5·654	5·173	4·234	3·765	4·875	5·310	4·475	5·122
(49)	August 26, ,,	6·376	6·676	5·482	5·825	5·883	5·597	5·608	5·974
50	September 24, ,,	5·860	5·675	5·530	4·658	4·040	3·674	3·159	2·980
51	October 24, ,,	2·928	3·185	2·759	1·751	2·226	2·661	2·255	2·530
52	November 22, ,,	2·907	2·571	2·209	2·306	2·775	2·402	1·808	1·895
53	December 22, ,,	2·224	3·158	2·849	2·470	2·676	2·334	3·009	4·024
54	January 20, 1852	4·223	3·239	2·884	2·610	2·702	2·495	2·953	5·150
55	February 19, ,,	4·887	2·679	2·439	3·434	3·674	3·077	3·400	3·903
(56)	March 20, ,,	3·788	3·571	3·823	4·037	5·379	5·013	4·772	5·026
(57)	April 19, ,,	5·373	6·242	6·243	5·788	5·561	5·533	4·383	4·664
(58)	May 18, ,,	5·837	6·215	5·601	5·379	5·299	5·459	4·818	5·631
(59)	June 17, ,,	6·415	5·700	5·139	5·288	5·357	5·343	4·876	4·452
(60)	July 16, ,,	5·339	6·270	6·123	5·054	4·854	4·904	5·711	6·375
(61)	August 15, ,,	6·479	6·810	6·672	5·503	5·402	6·123	5·551	5·494
(62)	September 13, ,,	5·444	5·063	3·731	3·777	4·017	3·828	3·576	3·955
63	October 12, ,,	2·666	2·993	3·799	3·319	2·998	2·460	2·300	2·211
64	November 11, ,,	2·335	2·280	2·380	2·678	2·678	3·387	3·124	3·525
65	December 10, ,,	3·994	2·994	2·162	2·513	3·722	3·599	2·815	3·035
66	January 9, 1853	2·723	2·431	2·656	2·816	3·360	3·249	2·633	2·761
67	February 7, ,,	2·278	2·164	2·610	2·484	2·188	2·383	2·657	2·789
68	March 9, ,,	3·365	2·815	2·907	3·873	4·137	3·262	3·445	4·884
(69)	April 8, ,,	5·872	5·590	5·109	4·715	4·263	4·147	4·726	5·424
(70)	May 7, ,,	4·727	3·857	4·441	5·699	5·928	5·310	4·840	5·184
(71)	June 6, ,,	5·299	6·037	6·357	6·191	5·917	5·755	5·894	5·895
(72)	July 5, ,,	5·580	5·688	5·688	5·919	6·363	5·315	5·184	6·237

Running No.	Lunation commencing new moon.	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(73)	August 4, 1853	6·123	6·221	5·837	5·528	6·523	7·187	6·787	7·291
(74)	September 3, ,,	8·035	6·948	6·536	7·196	6·821	6·500	5·505	5·093
75	October 2, ,,	3·868	3·166	2·672	3·022	2·725	2·599	2·553	2·322
76	,, 31, ,,	2·843	2·706	2·610	2·552	2·386	2·301	2·015	1·841
77	November 30, ,,	2·095	3·296	3·022	2·838	2·895	2·506	2·623	2·321
78	December 29, ,,	2·300	3·077	4·155	4·280	3·238	3·123	2·847	2·208
79	January 28, 1854	2·694	2·955	2·904	3·147	3·333	3·338	2·086	2·080
80	February 26, ,,	2·672	2·494	2·675	3·052	2·935	2·764	2·540	3·082
(81)	March 28, ,,	4·057	3·433	4·124	4·407	4·737	4·505	4·522	4·819
(82)	April 26, ,,	4·236	4·282	4·759	5·382	5·628	5·165	4·765	5·107
(83)	May 26, ,,	5·278	4·914	5·041	4·824	4·913	5·142	4·508	4·599
(84)	June 25, ,,	5·040	4·537	4·988	5·303	5·835	5·402	3·941	3·570
(85)	July 24, ,,	4·113	4·440	4·395	4·531	5·505	5·168	4·480	4·538
(86)	August 23, ,,	5·134	5·574	5·396	4·819	5·686	5·822	4·988	4·405
87	September 21, ,,	4·760	5·217	5·195	4·142	3·879	2·951	2·625	2·643
88	October 21, ,,	3·001	2·730	1·968	1·807	2·087	1·973	1·528	1·738
89	November 19, ,,	2·471	2·506	2·323	2·722	2·764	2·415	1·979	1·889
90	December 19, ,,	2·204	2·198	2·651	2·507	2·321	2·400	2·689	2·883
91	January 17, 1855	2·528	2·486	2·707	2·349	2·288	2·277	2·477	2·712
92	February 16, ,,	2·732	2·269	2·322	2·292	2·571	2·489	2·821	3·444
(93)	March 17, ,,	3·167	2·206	2·746	3·639	4·970	4·473	3·841	3·757
(94)	April 16, ,,	4·744	5·128	4·302	4·610	5·114	4·573	4·748	4·956
(95)	May 15, ,,	5·137	5·000	4·896	4·073	3·857	4·416	4·108	4·005
(96)	June 14, ,,	4·588	4·690	4·655	4·599	5·142	4·853	4·257	3·556
(97)	July 13, ,,	3·649	4·873	4·598	3·855	4·301	4·593	3·922	3·951
(98)	August 12, ,,	4·245	4·485	4·553	4·514	5·703	5·633	5·009	5·827
(99)	September 10, ,,	7·252	6·555	5·562	5·134	4·691	3·954	3·431	3·615
100	October 10, ,,	3·398	3·866	3·603	2·907	2·586	2·209	2·014	2·242
101	November 9, ,,	2·204	2·808	2·746	2·527	2·196	2·271	1·968	1·859
102	December 8, ,,	2·420	2·769	2·883	2·678	2·449	2·827	2·623	3·258
103	January 7, 1856	3·071	3·185	3·320	2·883	3·033	3·136	2·504	1·921
104	February 5, ,,	2·207	2·442	2·286	2·127	2·039	2·047	2·287	2·099
105	March 6, ,,	2·046	2·511	2·378	2·703	2·778	2·869	3·281	4·208
(106)	April 4, ,,	4·539	4·802	4·882	4·527	4·368	3·796	3·030	3·142
(107)	May 4, ,,	3·704	3·924	3·693	3·327	4·024	4·270	3·787	3·499
(108)	June 2, ,,	3·956	3·921	3·567	3·876	4·574	4·322	4·174	4·638
(109)	July 1, ,,	4·551	3·479	3·247	3·864	4·322	5·091	4·997	4·953
(110)	,, 31, ,,	5·030	4·551	4·585	5·488	5·776	5·516	4·734	4·734
(111)	August 29, ,,	5·225	5·996	5·397	5·461	6·298	6·072	4·939	4·196
112	September 28, ,,	4·162	3·396	2·389	2·497	2·881	2·881	2·675	2·607
113	October 28, ,,	1·880	2·195	1·926	1·646	1·693	1·694	1·988	2·360
114	November 27, ,,	2·367	2·648	2·916	2·182	2·355	2·456	2·172	1·727
115	December 26, ,,	2·135	2·987	3·267	2·824	2·321	2·510	2·173	2·161
116	January 25, 1857	2·310	2·072	2·607	2·923	2·813	2·332	2·173	2·184
117	February 24, ,,	2·149	2·058	1·829	2·081	1·852	1·726	1·898	3·087
(118)	March 25, ,,	3·087	2·305	2·745	3·444	3·087	2·957	3·967	4·024
(119)	April 23, ,,	4·185	4·226	4·255	4·883	5·043	5·088	5·203	5·672
(120)	May 23, ,,	5·626	4·820	3·974	3·685	4·631	5·207	4·379	3·924
(121)	June 21, ,,	3·671	3·897	4·454	4·522	4·516	5·042	4·619	4·070
(122)	July 20, ,,	4·128	4·208	3·590	4·445	4·733	5·050	4·775	3·944
(123)	August 19, ,,	5·122	5·603	5·282	4·760	5·797	6·765	6·221	6·202
124	September 17, ,,	6·877	6·455	5·637	4·962	4·871	4·276	3·785	3·005
125	October 17, ,,	2·758	2·687	2·355	2·085	1·761	1·578	2·023	2·435
126	November 16, ,,	2·704	3·218	2·698	2·149	2·006	2·339	2·498	2·407
127	December 15, ,,	4·539	4·036	1·969	2·375	2·950	2·964	4·008	4·013
128	January 14, 1858	3·648	2·939	2·733	3·030	3·316	2·836	2·332	2·264
129	February 13, ,,	2·664	2·847	2·538	2·978	3·609	3·115	3·408	3·979
130	March 15, ,,	4·745	4·654	4·082	3·774	4·323	4·309	5·477	6·051
(131)	April 13, ,,	5·297	4·313	4·265	5·180	5·448	4·665	4·881	5·996
(132)	May 12, ,,	5·914	5·324	5·248	5·969	5·443	5·306	5·019	4·556
(133)	June 11, ,,	4·413	4·665	4·836	4·654	5·111	6·289	6·095	5·134
(134)	July 10, ,,	4·836	5·259	4·871	5·026	6·009	6·254	5·580	5·145
(135)	August 8, ,,	6·449	6·838	5·991	6·339	6·696	6·504	5·912	6·450
(136)	September 7, ,,	5·900	5·801	6·103	5·340	5·957	6·613	6·048	5·365
137	October 6, ,,	4·413	3·293	3·499	3·064	3·121	4·036	3·789	2·026
138	November 5, ,,	2·470	2·985	2·733	2·940	2·928	2·012	1·932	1·496
139	December 4, ,,	2·196	2·786	2·321	2·470	2·607	2·566	3·293	3·773
140	January 3, 1859	3·472	3·145	3·328	3·554	3·156	3·842	4·860	3·755
141	February 2, ,,	2·509	3·362	3·292	2·493	2·344	3·453	3·519	2·525
142	March 4, ,,	2·758	3·500	3·945	5·230	5·314	4·336	4·413	5·180
(143)	April 2, ,,	4·848	4·871	6·552	7·067	7·490	7·698	6·066	5·797
(144)	May 2, ,,	7·342	7·850	7·296	6·834	7·341	8·055	5·104	5·625
(145)	,, 31, ,,	7·113	6·628	5·547	5·900	6·221	5·626	5·237	5·901
(146)	June 30, ,,	6·758	5·774	4·654	5·683	6·895	5·855	5·489	4·837
(147)	July 29, ,,	5·571	5·036	4·722	5·049	5·818	5·757	7·038	8·153
(148)	August 27, ,,	9·043	4·162	3·517	9·011	7·365	7·102	5·478	5·324
149	September 26, ,,	5·180	4·748	4·231	4·494	5·649	5·956	4·042	3·780

Running No.	Lunation commencing new moon.	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
150	October 25, 1859	3·802	3·819	3·282	2·710	3·030	2·658	2·352	2·579
151	November 24, ,,	2·653	2·573	2·298	3·361	3·853	5·284	4·289	3·046
152	December 23, ,,	3·114	2·621	3·376	3·281	2·744	3·076	3·454	3·557
153	January 22, 1860	3·213	3·133	3·327	3·142	3·259	3·541	2·813	2·882
154	February 21, ,,	3·143	2·470	2·209	3·310	4·693	4·700	3·476	3·458
(155)	March 22, ,,	4·769	4·914	5·467	5·751	5·037	4·858	4·803	4·918
(156)	April 20, ,,	5·032	5·216	5·799	5·947	6·199	6·261	6·124	6·450
(157)	May 20, ,,	5·846	4·859	5·635	6·035	6·610	6·450	5·684	5·407
(158)	June 18, ,,	6·370	7·274	7·525	8·289	8·657	7·045	5·167	4·802
(159)	July 18, ,,	6·256	6·908	5·867	6·560	7·095	8·242	9·320	8·330
(160)	August 16, ,,	8·440	8·166	8·097	7·918	9·057	8·462	7·399	5·696
(161)	September 14, ,,	5·410	5·833	6·713	6·805	6·427	6·461	4·620	3·898
162	October 14, ,,	4·278	4·008	3·170	2·610	3·306	3·418	2·425	2·299
163	November 12, ,,	2·005	2·018	2·196	2·375	2·498	2·197	2·071	2·883
164	December 12, ,,	2·710	2·306	2·539	2·553	2·649	2·832	2·855	2·924
165	January 10, 1861	2·723	2·928	3·191	3·786	3·374	3·054	2·776	2·445
166	February 9, ,,	2·699	2·618	2·733	2·902	3·416	3·691	3·065	2·786
167	March 11, ,,	3·651	3·923	3·705	4·186	5·404	5·404	5·730	5·512
(168)	April 9, ,,	4·677	4·318	3·792	4·584	5·329	5·352	4·963	5·101
(169)	May 9. ,,	4·483	4·426	4·643	5·078	6·299	6·385	5·665	5·490
(170)	June 8, ,,	5·158	5·855	6·106	5·523	6·186	6·026	5·032	5·015
(171)	July 7, ,,	6·719	7·554	5·653	4·312	4·904	5·600	5·851	7·096
(172)	August 6, ,,	6·939	6·964	6·585	5·341	7·436	8·228	7·509	6·641
(173)	September 4, ,,	7·520	6·683	5·895	6·690	7·041	6·539	5·580	4·971
174	October 3, ,,	4·394	4·670	3·937	3·093	2·853	2·372	3·057	3·070
175	November 2, ,,	2·758	3·030	2·825	2·276	2·481	2·154	2·161	2·140
176	December 1, ,,	2·684	3·331	2·984	3·064	3·190	3·373	2·154	2·222
177	,, 31, ,,	2·684	3·046	2·733	2·561	2·584	2·469	3·178	3·114
178	January 29, 1862	3·075	3·853	3·980	3·373	2·607	1·976	2·104	2·315
179	February 28, ,,	2·909	2·662	2·264	2·374	3·361	3·727	4·093	4·642
(180)	March 29, ,,	5·225	4·994	4·607	5·433	5·995	4·966	4·198	4·264
(181)	April 28, ,,	4·665	4·814	4·151	4·007	4·038	4·887	4·981	5·365
(182)	May 28, ,,	5·812	5·637	5·465	5·499	5·293	4·857	5·077	5·671
(183)	June 26, ,,	6·254	6·329	4·985	4·857	4·709	4·786	4·447	4·825
(184)	July 26, ,,	6·471	6·071	4·745	4·870	5·955	6·151	5·511	6·025
(185)	August 24, ,,	6·574	5·891	5·594	5·584	6·661	6·089	5·396	5·271
186	September 23, ,,	5·511	4·762	3·544	5·639	5·589	3·485	2·948	3·114
187	October 22, ,,	3·814	2·710	2·492	2·206	2·378	2·206	2·332	2·675
188	November 21, ,,	2·355	2·218	2·321	2·790	2·447	2·149	2·367	2·355
189	December 20, ,,	2·732	2·893	3·241	2·621	2·543	2·806	3·190	3·266
190	January 19, 1863	2·916	2·868	3·022	3·110	2·584	2·579	2·481	2·538
191	February 17, ,,	2·209	2·538	3·010	2·819	2·640	2·250	2·367	3·156
(192)	March 19, ,,	4·013	4·253	3·704	3·430	2·936	3·032	3·853	4·998
(193)	April 17, ,,	4·977	5·156	5·065	3·921	4·665	5·397	5·705	5·133
(194)	May 17, ,,	6·167	6·082	5·774	5·118	5·374	5·737	4·643	4·473
(195)	June 15, ,,	5·396	5·145	4·482	4·706	4·985	5·529	6·059	5·499
(196)	July 15, ,,	5·282	5·877	4·882	4·692	5·510	5·133	4·836	5·706
(197)	August 14, ,,	5·453	5·365	4·390	4·116	5·488	5·872	5·424	4·893
(198)	September 12, ,,	5·511	5·044	3·727	3·853	4·413	4·150	3·796	3·937
199	October 12, ,,	3·253	2·387	2·630	2·758	2·424	2·344	2·629	3·018
200	November 10, ,,	2·223	2·277	2·481	2·648	2·790	2·895	2·973	2·593
201	December 10, ,,	2·561	2·003	1·944	1·892	2·139	2·349	2·222	2·154
202	January 8, 1864	2·126	2·099	2·515	2·972	2·927	2·870	2·698	2·634
203	February 7, ,,	3·098	2·950	2·150	2·447	2·538	3·086	2·480	2·368
204	March 7, ,,	3·215	3·636	3·099	3·510	4·218	3·348	3·378	5·014
(205)	April 6, ,,	4·871	4·747	4·310	3·830	4·551	4·486	4·103	4·307
(206)	May 5, ,,	4·390	4·379	3·967	4·665	5·365	5·968	6·288	6·399
(207)	June 4, ,,	6·004	5·488	5·431	5·735	5·762	6·256	5·170	4·473
(208)	July 3, ,,	5·168	5·363	4·814	4·898	5·237	5·076	3·899	3·464
(209)	August 2, ,,	4·333	5·545	5·648	6·191	6·778	6·448	5·111	5·351
(210)	,, 31, ,,	6·833	6·334	6·265	6·476	6·036	5·427	4·797	3·956
211	September 30, ,,	3·586	2·786	2·875	3·183	3·080	2·964	2·836	2·618
212	October 30, ,,	2·470	2·031	1·635	2·072	2·538	2·100	1·669	1·864
213	November 28, ,,	2·275	2·222	2·035	2·195	3·110	3·236	2·755	2·662
214	December 28, ,,	2·182	2·864	3·759	3·471	3·178	3·807	3·461	2·864
215	January 26, 1865	2·866	3·312	3·358	2·915	2·058	2·832	3·392	2·785
216	February 25, ,,	1·838	1·880	1·989	2·675	3·430	3·279	3·830	3·316
(217)	March 26, ,,	2·858	2·881	2·893	3·647	4·196	4·418	4·413	4·692
(218)	April 25, ,,	5·740	4·528	4·047	4·836	4·745	4·569	4·825	4·373
(219)	May 24 ,,	4·913	4·885	4·061	4·253	4·893	4·950	4·665	4·973
(220)	June 22, ,,	5·042	5·385	4·493	4·791	4·642	4·002	3·602	3·361
(221)	July 22, ,,	4·002	4·253	5·419	5·968	5·442	5·351	4·447	4·157
(222)	August 20, ,,	4·905	4·816	4·756	5·159	5·065	4·448	4·036	4·653
223	September 19, ,,	5·019	5·453	5·019	4·157	3·716	3·501	3·443	3·544
224	October 19, ,,	3·677	2·497	2·630	3·350	3·647	3·238	3·419	3·030
225	November 17, ,,	2·389	1·841	2·332	2·332	2·355	2·207	2·287	2·515
226	December 17 ,,	1·944	1·962	2·580	2·950	3·224	2·835	2·710	3·247

Running. No.	Lunation commencing new moon.		(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
227	January 17,	1866	2·777	2·976	3·464	2·552	2·104	3·156	3·167	2·456
228	February 15,	,,	2·772	3·847	3·856	3·163	2·738	2·027	2·043	2·824
229	March 17,	,,	3·488	3·557	3·682	4·013	3·910	3·819	3·567	4·665
(230)	April 15,	,,	5·282	4·250	3·654	3·223	3·258	3·819	3·647	4·242
(231)	May 14,	,,	4·573	4·510	3·276	2·881	4·150	4·573	3·601	3·636
(232)	June 13,	,,	4·288	4·473	3·840	3·546	4·183	4·020	3·442	3·429
(233)	July 12,	,,	4·161	3·510	3·187	3·556	3·469	3·515	3·281	3·673
(234)	August 10,	,,	3·948	3·990	4·196	3·639	4·380	5·609	5·164	4·071
(235)	September 9,	,,	4·135	4·249	4·346	4·501	3·934	3·988	3·990	4·418
236	October 8,	,,	3·951	3·752	3·441	3·224	2·961	2·881	3·327	3·087
237	November 7,	,,	2·031	2·301	2·095	2·127	2·435	2·991	2·812	2·652
238	December 7,	,,	2·921	2·499	2·149	1·825	1·564	2·264	2·333	2·771
239	January 6,	1867	2·150	2·561	3·430	3·830	3·315	2·652	2·161	2·309
240	February 4,	,,	2·275	2·264	2·618	2·140	1·989	1·976	1·452	1·413
241	March 6,	,,	2·689	3·018	2·584	2·212	2·264	2·618	3·487	3·944
(242)	April 5,	,,	3·762	3·293	3·476	4·722	5·063	4·550	4·379	3·567
(243)	May 4,	,,	4·459	4·884	4·310	4·184	4·214	3·965	3·353	3·549
(244)	June 2,	,,	4·061	4·665	4·550	3·784	4·390	4·253	3·967	4·267
(245)	July 2,	,,	5·316	5·168	4·185	4·184	4·904	4·570	4·653	4·747
(246)	,, 31,	,,	5·271	4·865	3·871	4·377	4·157	4·859	4·951	4·379
(247)	August 29,	,,	4·814	5·265	4·453	3·727	4·516	4·157	3·887	3·718
248	September 28,	,,	3·636	3·403	3·350	2·824	2·572	3·064	3·201	2·961
249	October 27,	,,	3·389	2·346	1·921	2·035	2·332	2·298	1·830	1·932
250	November 26,	,,	3·373	2·847	2·024	2·182	2·149	2·044	2·092	2·414
251	December 26,	,,	2·099	2·031	1·550	1·811	2·127	2·104	1·966	1·592
252	January 25,	1868	2·069	2·085	2·103	2·730	2·366	2·133	2·012	2·188
253	February 23,	,,	2·017	1·995	1·898	2·135	2·867	2·908	2·761	2·915
(254)	March 24,	,,	3·367	3·348	3·693	4·191	4·644	4·624	5·202	5·900
(255)	April 23,	,,	5·802	5·275	4·830	4·678	5·013	4·665	4·110	4·950
(256)	May 22,	,,	5·241	4·236	3·939	4·973	5·562	5·288	5·059	5·097
(257)	June 20,	,,	4·796	4·651	4·041	3·813	4·419	4·579	4·036	4·116
(258)	July 20,	,,	4·613	4·779	3·670	3·578	4·035	3·979	4·167	5·131
(259)	August 18,	,,	6·883	6·476	5·808	5·631	6·551	6·763	6·357	6·391
260	September 16,	,,	5·608	4·534	3·739	3·416	4·773	4·281	4·001	2·648
261	October 16,	,,	2·415	2·709	3·264	2·550	1·989	1·674	1·498	1·722
262	November 14,	,,	2·178	2·687	2·372	2·401	2·510	2·511	1·932	1·955
263	December 14,	,,	2·383	2·201	1·996	1·723	2·346	3·293	2·892	2·389
264	January 13,	1869	2·246	2·795	3·407	3·238	3·438	3·164	3·028	2·810
265	February 11,	,,	2·319	1·675	2·132	2·744	2·717	2·675	2·498	2·827
266	March 13,	,,	2·999	2·721	3·167	3·756	4·813	4·985	5·025	5·162
(267)	April 12,	,,	5·214	3·821	3·904	4·720	4·413	4·034	4·059	4·756
(268)	May 11,	,,	6·551	6·528	5·842	5·762	5·063	5·362	5·636	5·848
(269)	June 10,	,,	6·963	7·299	5·882	6·318	6·471	5·528	5·842	5·708
(270)	July 9,	,,	5·768	5·570	5·928	6·922	6·843	6·163	5·694	5·722
(271)	August 8,	,,	6·414	6·934	5·945	4·836	5·810	6·202	5·728	5·022
(272)	September 6,	,,	6·746	8·348	7·340	6·185	5·196	4·676	4·082	4·081
273	October 5,	,,	4·270	4·102	3·310	3·030	2·498	2·830	2·893	2·366
274	November 4,	,,	2·401	2·367	2·326	2·595	2·578	2·284	2·389	2·010
275	December 3,	,,	2·704	2·309	2·412	3·081	3·127	3·430	3·327	2·881
276	January 2,	1870	3·855	3·996	2·681	2·641	2·315	2·435	2·561	2·881
277	,, 31,	,,	3·739	3·458	3·773	3·950	3·293	2·778	2·807	3·138
278	March 2,	,,	3·265	3·304	3·161	3·395	3·958	4·315	4·293	4·562
(279)	April 1,	,,	5·465	6·323	5·236	6·431	5·907	5·019	5·145	5·111
(280)	,, 30,	,,	5·482	4·527	4·625	5·608	7·060	6·980	6·263	6·784
(281)	May 30,	,,	6·814	6·088	5·808	5·722	7·146	8·386	5·871	5·825
(282)	June 29,	,,	7·397	6·448	5·659	6·201	7·409	7·285	7·283	6·854
(283)	July 28,	,,	6·334	7·217	7·008	6·839	6·921	7·992	7·169	7·798
(284)	August 27,	,,	8·380	7·980	7·192	7·357	7·117	7·403	7·855	7·895
285	September 25,	,,	7·443	7·484	6·660	4·825	4·459	4·916	4·350	3·965
286	October 24,	,,	5·241	4·270	3·127	3·350	3·716	3·316	3·258	3·073
287	November 23,	,,	2·789	2·470	2·115	2·389	2·532	2·098	2·389	2·789
288	December 22,	,,	2·653	2·785	2·687	3·306	3·601	3·718	3·887	3·636
289	January 21,	1871	2·967	3·156	2·761	2·967	2·996	3·087	4·087	3·841
290	February 19,	,,	2·565	2·738	3·076	2·401	3·883	3·896	4·202	4·390
(291)	March 21,	,,	4·791	5·282	4·870	4·308	4·836	5·839	6·031	6·854
(292)	April 20,	,,	7·180	6·088	5·757	4·617	5·111	5·682	5·757	5·980
(293)	May 19,	,,	6·665	5·995	5·392	6·092	6·626	6·819	6·814	6·562
(294)	June 18,	,,	5·602	5·105	5·648	6·220	7·151	7·117	6·151	5·528
(295)	July 17,	,,	6·557	6·871	5·585	5·728	5·989	6·345	6·648	7·203
(296)	August 16,	,,	8·100	8·321	7·157	6·565	7·500	7·563	7·546	7·769
(297)	September 15,	,,	6·945	6·397	5·413	3·876	5·402	5·362	5·019	4·871
298	October 14,	,,	4·047	3·073	3·401	3·519	3·139	3·859	3·413	3·041
299	November 12,	,,	2·922	2·154	2·630	2·595	2·435	2·361	2·126	2·099
300	December 12,	,,	2·898	3·128	2·315	2·305	2·374	2·476	3·375	3·653
301	January 10,	1872	3·510	3·011	2·669	2·915	2·858	3·178	6·042	7·265
302	February 9,	,,	4·345	3·341	2·841	2·675	2·721	3·538	4·482	4·493
303	March 9,	,,	4·356	4·133	4·550	5·099	4·891	5·016	3·622	3·485

Running No.	Lunation commencing new moon.		(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(304)	April 8,	1872.........	5·036	5·185	5·534	6·325	6·562	6·922	5·745	5·762
(305)	May 7,	,, ...	6·431	6·814	6·082	6·139	7·127	6·832	5·894	5·202
(306)	June 6,	,, ...	5·168	6·157	5·837	5·824	5·734	4·651	4·368	5·145
(307)	July 5,	,, ...	5·774	7·443	5·579	5·162	5·888	5·694	5·797	6·071
(308)	August 4,	,, ...	6·957	7·374	6·545	5·687	5·825	5·763	5·985	6·717
(309)	September 3,	,, ...	7·380	7·347	7·032	7·114	7·620	6·860	6·037	6·380

6. The whole series of 309 lunations of Table III gives the following results :—

Phase of lunation	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Value of range	4·34	4·30	4·07	4·11	4·34	4·29	4·06	4·12	(A)

a series which like that of Kew has a decided double period, having maxima about new and full moon and minima about first and last quarter. The sum of the four left-hand numbers (16·82) is the same (sensibly) as the sum of the four right-hand numbers (16·81).

The winter lunations by themselves, of which there are 153, give—

Phase of lunation	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Value of range	3·14	3·05	2·95	3·01	3·09	3·00	2·97	3·03	(B)
Correction applicable to winter months	− ·08	− ·04	− ·01	+ ·01	+ ·03	+ ·03	+ ·02	·00	
Corrected value of winter lunar range	3·06	3·01	2·94	3·02	3·12	3·03	2·99	3·03	(C)

and the summer lunations by themselves, of which there are 156, give—

Phase of lunation	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Value of range	5·52	5·52	5·18	5·18	5·56	5·56	5·13	5·19	(D)
Correction applicable to summer months	+ ·07	+ ·03	− ·01	− ·02	− ·03	− ·03	− ·03	·00	
Corrected value of summer lunar range	5·59	5·55	5·17	5·16	5·53	5·53	5·10	5·19	(E)

The series (A), (C), and (E) are curved in figs. 2, 3, and 4. The three curves are all double waves of regular and similar form and agreeing in phase, and the two waves have about the same amplitude; but the range of the winter curve is much less, and that of the summer curve much greater, than that of the annual curve. The summer variation is very similar to that of Kew, but the winter variations, though agreeing in being of less range than the respective summer variations, are quite unlike each other, that of Kew being mainly a single period variation.

D′. *Possible Variations of the Lunar Effect with the Sun-spot Period.*

7. The groups of years taken, in this case, as years about the times of maximum sun-spots, and as years about the times of minimum sun-spots, were the same as were used in the similar inquiry respecting temperature-ranges (paragraph 12), except that here the records under discussion stop at 1871·75 and both sets of winters include that of 1870·75 to 1871·75. The results given by the lunations of the first set of winter half-years are—for minimum and maximum sun-spot times respectively :—

Phase of lunation	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Winter variation (minimum period)	2·83	2·89	2·84	2·77	2·74	2·66	2·63	2·76	(F)
Deduct (B)	3·14	3·05	2·95	3·01	3·09	3·00	2·97	3·03	
Supposed effect of solar minimum	−0·31	−0·16	−0·11	−0·24	−0·35	−0·34	−0·34	−0·27	(G)

and

Phase of lunation	(0)	(1)	(2)	(3)	(4)	(5	(6)	(7)	
Winter variation (maximum period)	3·35	3·19	2·98	3·04	3·22	3·29	3·27	3·18	(H)
Deduct (B)	3·14	3·05	2·95	3·01	3·09	3·00	2·97	3·03	
Supposed effect of solar maximum	+0·21	+0·14	+0·03	+0·03	+0·13	+0·29	+0·30	+0·15	(I)

and those given by the second set of winter half-years are :—

Phase of lunation	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Winter variation (minimum period)	2·93	2·90	2·77	2·77	2·77	2·71	2·70	2·81	(F′)
Deduct (B)	3·14	3·05	2·95	3·01	3·09	3·00	2·97	3·03	
Supposed effect of solar minimum	−0·21	−0·15	−0·18	−0·24	−0·32	−0·29	−0·27	−0·22	(G

and

Phase of lunation	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Winter variation (maximum period)	3·30	3·21	3·07	3·10	3·27	3·35	3·31	3·21	(H′)
Deduct (B)	3·14	3·05	2·95	3·01	3·09	3·00	2·97	3·03	
Supposed effect of solar maximum	+0·16	+0·16	+0·12	+0·09	+0·18	+0·35	+0·34	+0·18	(I′)

The selected lunations of the summer half-years give—for minimum and maximum sun-spot times respectively :—

Phase of lunation	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Summer variation (minimum period)	4·83	4·77	4·39	4·46	4·83	4·85	4·50	4·47	(J)
Deduct (D)	5·52	5·52	5·18	5·18	5·56	5·56	5·13	5·19	
Supposed effect of solar minimum	−0·69	−0·75	−0·79	−0·72	−0·73	−0·71	−0·63	−0·72	(K)

and

Phase of lunation	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	
Summer variation (maximum period)	6·11	6·17	5·79	5·79	6·23	6·21	5·77	5·81	(L)
Deduct (D)	5·52	5·52	5·18	5·18	5·56	5·56	5·13	5·19	
Supposed effect of solar maximum	+0·59	+0·65	+0·61	+0·61	+0·67	+0·65	+0·64	+0·62	(M)

8. The series (G), (I), (G′), (I′), (K), and (M), are curved in figs. 5 to 10 in order, and their respective mean values are − ·26, +·16, −·24, +·20, −·72 and +·63. These numbers imply that the result already found, that years about the time of maximum sun-spots have a larger declination-range than years about the time of minimum sun-spots, holds also for the winter and summer half-years separately.

The amplitude of the variation corresponding to the sun-spot variation is like the absolute declination-range, and even in greater proportion, much greater in summer than in winter. On the other hand, the supposed sun-spot effect on the lunar variation is greater in winter than in summer, the winter curves (figs. 5 to 8) being much bolder than the summer curves (figs. 9 and 10): the former are, indeed, of greater range and equally definite, though different, in character with the absolute winter variation shown in fig. 2.

E. *Variations which seem to depend on Planetary Configurations.*

9. The periods chosen for examination with respect to variation of declination-range are the same as were selected by Dr. Stewart in our model paper, viz: (α) the period of conjunction of Venus and Mercury, (β) the solar period of Mercury, and (γ) the period of conjunction of Mercury and Jupiter. Assuming with Dr. Stewart, that as the periods differ little from three months,* three-monthly values of the phenomenon will be nearly free from any inequality depending on the periods, and that differences between the monthly and the three-monthly values will exhibit any such inequality as may exist, we have subtracted the three-monthly values from the monthly values referred to

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* The assumption, as regards the period of conjunction of Venus and Mercury, is not very exact, and this may be the reason why the variation found in this case is of less simple character than that of the other two periods.

in paragraph 3, thus obtaining a table of differences (48 to each year) to be re-arranged successively in three new tables, in lines corresponding to the respective periods—one line to each period.

The dates of conjunction and perihelion respectively having been taken from the "Nautical Almanac," the differences were divided into sets in such a manner that the mean dates of the first and last differences of each set never encroached on the preceding or following period. The number of differences in a set were, for the periods (β) and (γ), always either twelve or eleven, and when only eleven they were increased to twelve by repeating either the last difference of the preceding set or the first difference of the following set, whichever was the nearer in point of time to the intervening period. The period (α) is so variable that the number of differences in a set varied from seventeen to twenty-one; when seventeen they were increased to nineteen by repeating the preceding and following differences; when eighteen they were increased to nineteen in the same manner as eleven were increased to twelve in the case just described; when twenty the two first or two last numbers were replaced by the mean of them—whichever was furthest in point of time from the middle of the intervening period; and when twenty-one the two first and the two last differences were both replaced by their mean values.

The results (the algebraical sums of the columns) given by the several periods are as follows:—

Table IV.—Period of Conjunction of Venus and Mercury (0° denotes Conjunction).

	62 periods.	First 31 periods.	Last 31 periods.
Between 0° and $(\frac{360}{19})^\circ$	+839	+637	+202
„ $(\frac{360}{19})$ „ $2(\frac{360}{19})$	+741	+453	+288
„ $2(\frac{360}{19})$ „ $3(\frac{360}{19})$	−221	− 87	−134
„ $3(\frac{360}{19})$ „ $4(\frac{360}{19})$	−545	−331	−214
„ $4(\frac{360}{19})$ „ $5(\frac{360}{19})$	−564	−595	+ 31
„ $5(\frac{360}{19})$ „ $6(\frac{360}{19})$	−267	−604	+337
„ $6(\frac{360}{19})$ „ $7(\frac{360}{19})$	+448	−261	+709
„ $7(\frac{360}{19})$ „ $8(\frac{360}{19})$	+398	+ 46	+352
„ $8(\frac{360}{19})$ „ $9(\frac{360}{19})$	+ 51	+130	− 79
„ $9(\frac{360}{19})$ „ $10(\frac{360}{19})$	−700	−347	−353
„ $10(\frac{360}{19})$ „ $11(\frac{360}{19})$	−311	+120	−431
„ $11(\frac{360}{19})$ „ $12(\frac{360}{19})$	+252	+388	−136
„ $12(\frac{360}{19})$ „ $13(\frac{360}{19})$	+637	+390	+247
„ $13(\frac{360}{19})$ „ $14(\frac{360}{19})$	+855	+484	+371
„ $14(\frac{360}{19})$ „ $15(\frac{360}{19})$	−264	−261	− 3
„ $15(\frac{360}{19})$ „ $16(\frac{360}{19})$	−976	−516	−460
„ $16(\frac{360}{19})$ „ $17(\frac{360}{19})$	−426	− 91	−335
„ $17(\frac{360}{19})$ „ $18(\frac{360}{19})$	+ 81	+282	−201
„ $18(\frac{360}{19})$ „ $(\frac{360}{19})$	+196	+234	− 38

Table V.—Period of Mercury about the Sun (0° denotes Perihelion).

	104 periods.	First 52 periods.	Last 52 periods.
Between 0° and 30°	−1133	− 64	−1069
" 30 " 60	− 538	+193	− 731
" 60 " 90	− 37	+155	− 192
" 90 " 120	+ 142	−230	+ 372
" 120 " 150	+ 843	−167	+1010
" 150 " 180	+ 953	−117	+1070
" 180 " 210	+ 512	−176	+ 688
" 210 " 240	+ 375	− 71	+ 446
" 240 " 270	− 22	+179	− 201
" 270 " 300	− 484	+175	− 659
" 300 " 330	− 389	+ 63	− 455
" 330 " 360	− 828	+138	− 966

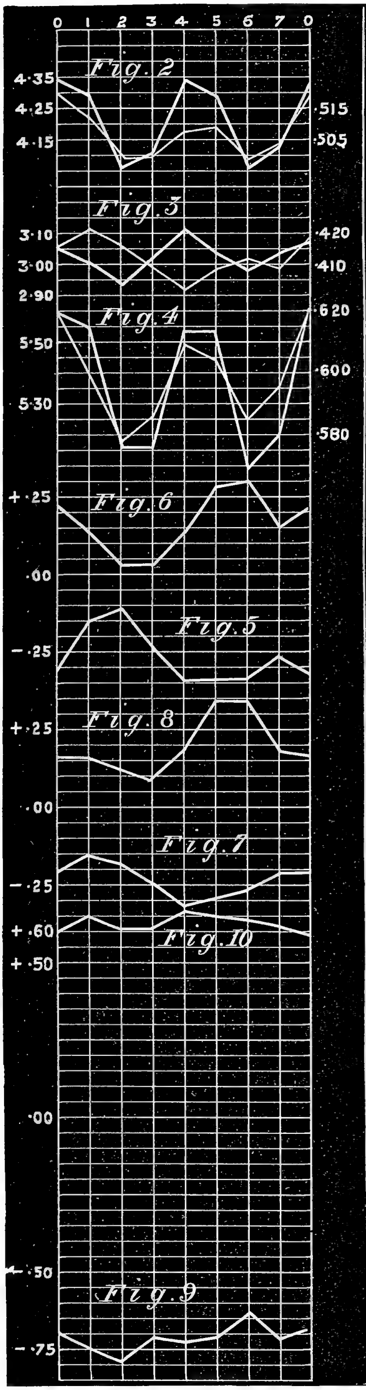
Table VI.—Period of Conjunction of Mercury and Jupiter (0° denotes Conjunction).

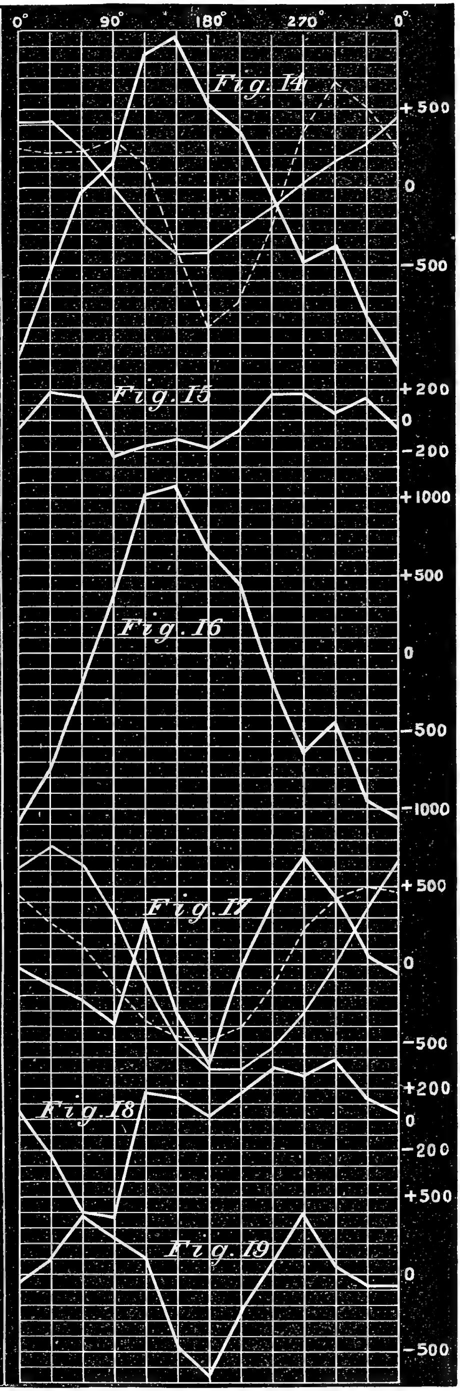
	102 periods.	First 51 periods.	Last 51 periods.
Between 0° and 30°	− 34	+ 50	− 84
" 30 " 60	−130	−223	+ 93
" 60 " 90	−224	−602	+378
" 90 " 120	−395	−628	+233
" 120 " 150	+285	+163	+122
" 150 " 180	−350	+132	−482
" 180 " 210	−651	+ 10	−661
" 210 " 240	− 73	+158	−231
" 240 " 270	+401	+318	+ 83
" 270 " 300	+690	+291	+399
" 300 " 330	+415	+389	+ 26
" 330 " 360	+ 57	+136	− 79

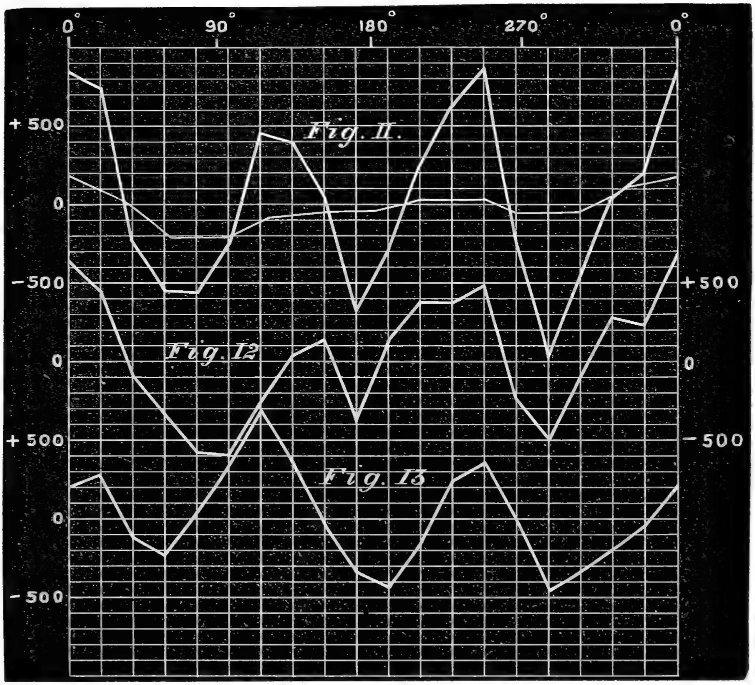
The numbers in the several columns of Tables IV, V, and VI are graphically represented by figs. 11 to 19 in order.

10. The most marked feature in the Venus and Mercury period is a treble wave which repeats itself consistently in both halves of the series of observation, and has one of its maximum values at the time of conjunction. The variation given by the solar period of Mercury is nearly all due to the last 52 periods: it is represented by a single wave, which is similar to the corresponding Kew curve when inverted. The curves of the first 51 and last 51 periods of the conjunction of Mercury and Jupiter are very unlike each other, and they are also unlike the corresponding curves for Kew and Trevandrum.

The several curves in weak and interrupted lines for Kew and Trevandrum repectively are made for comparison with those for Bombay, which are drawn strong. In the planetary results it must


0 1 2 3 4 5 6 7 0
Fig. 2
4·35
4·25
4·15
·515
·505
Fig. 3
3·10
3·00
2·90
·420
·410
Fig. 4
5·50
5·30
·620
·600
·580
Fig. 6
+·25
·60
Fig. 5
−·25
Fig. 8
+·25
·00
Fig. 7
−·25
+·60
Fig. 10
+·50
·00
·50
Fig. 9
−·75


0° 90° 180° 270° 0°
Fig. 14
+500
0
−500
Fig. 15
+200
0
−200
Fig. 16
+1000
+500
0
−500
−1000
Fig. 17
+500
0
−500
Fig. 18
+200
0
−200
Fig. 19
+500
0
−500



be observed that the variations are for different numbers of periods at the different stations as follows:—

	Bombay.	Kew.	Trevandrum.
Mercury and Venus	62	39	
Mercury..................	104	65	47
Mercury and Jupiter	102	63	43

11. It may be remarked that, as at Kew, there is some resemblance between the curves of lunar variation of temperature-range and declination-range, and that it exhibits itself both in summer and in winter separately. [Compare figs. 2, 3, and 4 of declination-ranges with figs. 2, 3, and 4 of temperature-ranges.] Comparing also fig. 1 of declination-ranges with fig. 1 of temperature-ranges, there is seen to be a remarkable inversion of movement between 1859 and 1872, but the same rule does not hold good for the earlier years.

November 16, 1882.

THE PRESIDENT in the Chair.

In pursuance of the Statutes, notice of the ensuing Anniversary Meeting was given from the Chair.

Professor Valentine Ball, Mr. Charles Baron Clarke, Mr. Richard Tetley Glazebrook, and Professor John C. Malet were admitted into the Society.

General Boileau, Mr. W. H. M. Christie, Mr. W. De La Rue, Mr. G. Matthey, and Dr. W. J. Russell, having been nominated by the President, were elected by ballot Auditors of the Treasurer's Accounts on the part of the Society.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Nerves of the Frog's Lung." By WILLIAM STIRLING, M.D., Sc.D. Communicated by Professor T. H. HUXLEY, F.R.S. Received June 17, 1882.

(Abstract.)

The author has re-examined the lung of the frog with special reference to its nervous apparatus. Arnold, several years ago, gave a description of the nerves of the frog's lung. Amongst the nerve-fibres he found bell-shaped nerve-cells provided with a straight and a spiral fibre. He was unable to find any nerve-cells in the apex of the lung. Kandarazki has more recently examined these nerves. He was unable to find any trace of a spiral fibre in the nerve-cells, and he considers the appearances which have been figured as such, to be due to folds in the capsule of the cell. After giving an account of the methods used for exhibiting the course, relations, and structure of the nerves of the lung, the author gives an account of the result of his observations.

The pulmonary branches of the vagus enter the lung at its root or near its attached end. The branches contain medullated and non-medullated nerve-fibres, and amongst these fibres before they enter the

lung are ganglionic cells. More than forty medullated nerve-fibres were counted as they entered the lung, but they were even still more numerous, while in addition there were many non-medullated fibres whose number it was not so easy to ascertain. The main trunks of the nerve were traced into the lung under the serosa, where they give off larger and smaller branches—containing medullated and non-medullated fibres—which could be traced across the alveolar wall, giving off finer branches in their course. The main trunks exchange a few nerve-fibres, but the number of fibres so exchanged is not large. Many of the finer branches, which may contain just one or two medullated fibres, could be traced to the muscular septa of the alveolar wall; they dip into it, lose their myeline, and form a plexus of non-medullated fibres with elongated meshes. From this plexus finer branches are given off which dip down between the non-striped muscle-cells. The non-medullated nerves join and form a wide-meshed plexus of nerve-fibrils upon the alveolar wall in relation with the thin layer of non-striped muscle which occurs there. From this plexus fibres are given off which seem to form a second plexus with finer meshes. This plexus is quite distinct in its characters from that which occurs in the alveolar septa. Some non-medullated fibres occur on the walls of the blood-vessels. The nerve-cells which occur along the course of the nerve-fibres are very numerous. They are most numerous where a branch is given off from one of the main trunks, but they also occur along the course of the nerves, it may be at the side or amongst the nerve-fibres, but always within the sheath of the nerve. These nerve-cells present the characters of the cells described by Arnold and Beale, and in the fully developed condition at least are provided with a straight and a spiral process. They are to be found even in the nerve-branches in the apex of the lung. More than three hundred nerve-cells were counted in each lung disposed amongst the nerve-fibres, so that they are relatively far more numerous than the medullated nerve-fibres which enter the lung. The arrangement of the nerve-fibres which supply the muscular coats of the arteries and veins are then described and figured. The paper is illustrated by accurate drawings, which show the exact distribution of the intra-pulmonary nerves, the structure of the nerve-cells, and the plexuses of nerve-fibrils which occur in relation with the muscular coat of the lung and the pulmonary blood-vessels.

II. "Notice of Portions of the Skeleton of the Trunk and Limbs of the Great Horned Saurian of Australia (*Megalania prisca*, Ow.)." By Professor OWEN, C.B., F.R.S., G.S., &c. Received June 19, 1882.

Former communications on the subject, admitted into the "Philosophical Transactions," related to the skull and terminal caudal vertebræ, from a river-bed at Toowoomba, Queensland; and in the discussion on those papers, a doubt was expressed whether they had been correctly referred to the genus and species founded upon trunk-vertebræ from other and remote localities, which had been the subject of a former paper, communicated to the Royal Society June 17th, 1858, and published in the volume of the "Transactions," 1858, p. 48, Plates VII and VIII.

My correspondence with Dr. Bennett, F.L.S. of Sydney, New South Wales, and with his son, George Frederic Bennett, Esq., of Toowoomba, Queensland, has been unintermitting; and I have been recently favoured with the reception of a consignment of fossil remains from the petrified drift formation of the same river-bed in which the subjects of the "communications" of April 15th, 1880, and of February 3rd, 1881, were found.

The most instructive of these is a dorsal vertebra, so closely corresponding in size and characters with the subject of Plate VII (*tom. cit.* 1858) as to render further description and figures unnecessary.

Coming from the same formation, locality, and vicinity, as the parts of the skull (restored in Plates 37, 38, of the "Phil. Trans.," 1880), no further doubt as to the Saurian affinities of that part seems admissible. No trace of a Chelonian reptile has been found or indicated by any of the numerous fragmentary remains of *Megalania* from Mr. Bennett's locality of that genus.

In the last transmission, received June 16th, 1882, are portions of the pelvis, including part of the sacrum and of the right iliac bones. The sacral fragment indicates two vertebræ, with the foramina for the transmission of the nerves of the hind limbs, in two pairs. The corresponding vertebræ have coalesced, and there is a small portion of a third vertebra in like osseous attachment.

The portion of a right iliac bone shows a greater relative expanse of the upper part than is seen in existing *Lacertilia*, and a corresponding departure from the cylindroid character of the body of the ilium in *Chelonia*. On the probable hypothesis that the above fossil fragments, like the associated dorsal vertebra, are parts of the skeleton of *Megalania prisca*, the pelvic fragments suggest an approach to a Dinosaurian character.

Of the corresponding limb a fragment of the shaft indicates a large relative size. Not any of the above named fossils appear to me to call for a Plate.

III. "On the Relation of particular Structural Features in certain Leaves to the Phenomena of Nyctitropism and Movements incident on Stimulation by Concussion." By D. D. CUNNINGHAM. Communicated by Sir J. HOOKER, K.C.S.I., F.R.S. Received June 29, 1882.

(Abstract.)

The leaves forming the subjects of special study were those of the following species:—*Mimosa pudica*, *Neptunia oleracea*, *Pithecolobium Saman*, *Albizzia Lebbek*, *Leucaena glauca*, *Cassia alata*, and *Cassia sumatrana*. In all of them nyctitropic movements occur, and in all, save those of *Albizzia Lebbek*, in which the point has not yet been determined, movements also follow stimulation by concussion. The movements in individual instances, however, differ greatly in character, in degree, and in persistence in relation to the life of the leaves. The differences in regard to the rapidity and extent of movement incident on concussion are specially marked. In *Mimosa pudica* and *Neptunia oleracea* such movements are very extensive and rapid; in the two species of *Cassia* they are very slight and gradual, and those in the leaves of *Pithecolobium Saman* and *Leucaena glauca* are of an intermediate character. In some cases both classes of movements persist from the period at which they first manifest themselves until the death of the leaves, but in others they diminish with increasing age, and disappear long ere death ensues.

The paper is divided into three principal sections. The first of these deals with the structural features of the individual leaves, the second with the phenomena of movement, and the third contains a comparison of the structural and motorial phenomena with a view to determine how far these are essentially related to one another.

In the first section it is shown that the contractile organs which are the chief determinants of movement are, throughout the entire series of leaves, specially characterised by the porous nature of their component tissues. The porosity is very various in degree in different cases, and, according to the extent to which it prevails, converts the entire pulvinar organs, to a greater or less degree, into masses of a spongy texture, specially fitted to allow of the ready redistribution of fluid contents. In those cases where it is most highly developed, as in *Mimosa pudica* and *Neptunia oleracea*, the pulvinar parenchyma is composed in greater part of finely porous cells, and in some portions contains masses of cells which, in addition to the fine pores, are

provided with one or more large ostiola—rounded openings with thickened margins. Here, also, an enormously developed system of very large intercellular spaces is present in the deeper layers, communicating freely on one hand with the cavities of the porous cells and on the other with a system of channels traversing the epidermal tissues of the petiolar organs. In less developed cases, such as those of the other species treated of, pores alone are present in the cells, ostiola are entirely absent, and the system of intercellular spaces is feebly represented or almost entirely suppressed. The vascular bundles are in all cases characterised by an abundance of porous elements, both in the bast and very especially in the wood, where a system of large porous ducts almost entirely replaces the spirals of the axial and petiolar tissues.

Another very important structural feature in the contractile organs is the existence of very marked inequalities in the strength of the formed elements of the tissues of different areas, as well as local variations in the amount of protoplasm and specially of chlorophyll corpuscles in different portions of the superficial parenchyma.

In the second section of the paper special attention is drawn to the fact that the maximum nocturnal position appears in all cases to be normally attained at a relatively early period after the onset of darkness, and that, after this, a reverse movement, tending more or less to the resumption of the diurnal position, sets in independent of any renewed incidence of light. In connexion with this it must be noted that the movements incident on concussion by stimulation tend towards the assumption of the initial nocturnal position.

The third section of the paper contains an attempt to demonstrate the existence of an essential connexion between the special structural and motorial phenomena in individual leaves. It is pointed out that the occurrence of movement incident on stimulation by concussion is in all these cases associated with the existence of structural arrangements in the contractile organs specially calculated to facilitate redistribution of fluids in the tissues, and that the rapidity and magnitude of the movements in individual cases bear a direct relation to the degree of development of such structural features. For example, in *Mimosa pudica* we find very rapid and extensive movements associated with extreme porosity of the cells of the pulvinar parenchyma, and with the presence of a very highly elaborated system of intercellular spaces. In other cases, such as that of *Pithecolobium Saman*, we have more limited and gradual movement along with less elaboration of porosity and diminution of the intercellular system, and in the case of the two species of Cassia we encounter minimum development of the special structural features with minimum development of concussion movements. This appears to indicate that rapidity and extent of concussion movement may be ascribed rather to structural than functional peculiarities in the tissues. The movements in such cases do not necessarily imply the presence of protoplasm specially endowed with sensitive and contractile power proportionate to the magnitude and rate of movement; they rather appear merely to indicate the degree to which structural arrangements permitting of movement are present. They appear to indicate the degree to which arrangements permitting of massive redistribution of fluids throughout the tissues at large, as distinguished from mere redistribution of cell contents, such as may occur in any tissue composed of closed elements, are present. In other words, the movements may be mere indices to the common sensitive and contractile function of vegetable protoplasm, appearing in dependence on certain structural peculiarities of tissue, and not indices to the presence of specially endowed protoplasm.

While, however, structural peculiarities of tissue, according to this view, appear to be essential determinants of the occurrence of movements, the presence of a contractile and sensitive protoplasm is, of course, necessary in order to the actual occurrence of movement—in order that the apparatus for redistribution of fluid, and consequent redistribution of tension throughout the tissues, shall be called into play. In connexion with this, too, it is important to bear clearly in mind that, while porosity of the tissue elements appears in these cases to be their most important structural feature in relation to the occurrence of movement, an extreme porosity of tissue may coexist with an entire absence of movement even where an active protoplasm is still present. For example, in mature leaves of *Albizzia Lebbek* movements are entirely suppressed, although the porosity of tissue remains unimpaired, the abolition of movement being associated with thickening and altered consistence of the cell-walls of the pulvinar parenchyma, increased lignification of the vascular bundles, and excessive cuticularisation of the epidermis. In order to the normal occurrence of movement there must, then, be an active protoplasm, and conditions of structure not merely providing means for ready transfer of fluid, but allowing of ready alterations in the calibre of the tissue elements. In connexion with the subject of concussion movements, it is also pointed out that in those cases where a distinct propagation of stimulation from one part to another, as in *Mimosa pudica*, occurs, special structural means are present, which may serve as a mechanical substitute for a nervous conducting apparatus. These means consist of a continuous system of definite air-passages uniting the intercellular spaces of the different contractile organs, and calculated to serve as channels for the propagation of impulses arising from the sudden injection of liquids from the cells of the contractile tissue into the spaces normally occupied by air.

In regard to nyctitropic movements, it is shown that the parenchyma of the pulvinar organs consists of portions differing from one another in mechanical and physiological strength, the mechanically strong ones being characterised by the nature and amount of formed materials, the functionally strong ones by relative excess of protoplasm, and specially of chlorophyll corpuscles. Every pulvinus, in fact, consists of different portions, in some of which functional, and in others mechanical power is present in excess, and, as from their localisation the functionally and mechanically strong portions are opposed to one another, the position of the leaf as determined by the pulvinus must vary with the relative strength of these. But it can be further shown that the mechanically stronger portions constantly tend to cause the leaf to assume the nocturnal position, while the physiologically strong ones act in the reverse direction. Hence, there must be constantly recurring fluctuations in the relative strength of the different areas, corresponding with the recurring variations in position of the leaves. But these variations in relative strength must be ascribed rather to changes occurring in the physiologically strong than in the mechanically strong portions of tissue, to elevations and depressions of strength connected with functional rather than structural conditions. The functionally stronger areas, accordingly, must be regarded as constantly becoming relatively stronger diurnally and weaker nocturnally, and this recurring fluctuation of strength may be fairly ascribed to variations in tension, due to variations in the absorption of liquids connected with the specific diurnal functional activity of the protoplasmic elements of the tissues, and specially of the chlorophyll corpuscles under the influence of light.

When the varying kinds and degrees of nyctitropic movements exhibited by the leaves of the individual species at various stages of their development are compared with the coexisting structural features, additional evidence is found of the essential dependence of the former on the latter. The positions of the leaves in the bud and at later stages, and the assumption and persistence of special movements, are clearly related to the varying degrees of development and the condition of particular portions of the contractile organs.

The phenomenon of reverse movement towards the diurnal position during the latter portion of the night, and independent of any renewed incidence of light is not necessarily opposed to a belief in inequalities of functional activity and tension of particular areas of the pulvinar tissues under the influence of light as the essential cause of nyctitropic movements. The nocturnal reversion may be ascribed to another agency than a rise in functional distension independent of light. It may arise from alterations in relative tension of the different portions of tissue due to differences in structure. Those areas in which diurnal functional activity and the incident increased absorption and tension must attain a maximum are also those in which the greatest facilities for the redistribution of fluids are provided by the nature of the structure. Granting that light is the cause of distension proportionate to functional activity and in excess of the passive properties of the tissue, the pulvinar tissues throughout during the incidence of light will contain a functional excess of liquid, and the withdrawal of light will lead to a general diminution of distension—the non-illuminated tissues parting with the functional excess of fluid, and retaining merely the amount proportional to their diminished activity. But those portions of tissue providing the greatest facilities for redistribution of fluids must, other things being alike, tend most rapidly to arrive at their passive condition of tension. In the pulvini, then, it is quite possible that those portions of tissue which are characterised by excessive diurnal activity and functional distension may attain their passive condition sooner than the others do, due to their respective structural peculiarities. But from the period at which the one part of the pulvinus has attained its passive condition until the other has also done so, a continuous alteration in their relative tensions must take place. Those portions of tissue which tend to the assumption of the diurnal position will, according to this, on the onset of darkness, rapidly attain their maximum relative weakness, and after this will become relatively stronger until the opposing portions have also arrived at their passive condition of tension. An early development of the maximum nocturnal position, and a subsequent reversion towards the diurnal one, such as actually occurs, might therefore be looked for on *a priori* grounds. Nocturnal reversion may probably in great measure be thus explained, but, as indeed in the case of other movements, other factors such as variations in the absolute weight and consequent leverage of various parts of the leaves connected with variations in amount of respiration and assimilation no doubt come more or less into play.

IV. "On the Continuity of the Protoplasm in the Motile Organs of Leaves." By WALTER GARDINER, B.A., late Scholar of Clare College, Cambridge. Communicated by Dr. M. FOSTER, Sec. R.S. Received November 11, 1882.

In a preliminary note published in the "Quarterly Journal of Microscopical Science," for October, 1882, I stated that I had succeeded in demonstrating that the continuity of the protoplasm of adjacent cells in the pulvinus of *Mimosa pudica* was maintained by protoplasmic filaments, which passed through pits in the cell wall. I have since then shown that the same occurs in *Robinia* and *Amicia*. In *Phaseolus* the connexion is much less pronounced, and as yet I must withhold any definite decision upon it. I cannot, however, doubt that protoplasmic continuity is of very frequent occurrence, both in pulvini, stems, roots, and tubers, and I regret that some short time must elapse before I can publish any detailed account of my further researches.

Since the winter season was rapidly advancing, I instituted other experiments with fresh material, with a view to confirm, if possible, results obtained by strong reagents, whose action was necessarily attended with grave alteration of the tissues. As a consequence of these experiments, some new and important facts bearing on the phenomenon of Plasmolysis have come to light. Hugo de Vries, in his work on cell turgescence, has shown that by treating fresh uninjured cells with progressively stronger and stronger solutions of a neutral salt, *e.g.*, 4 per cent. and 6 per cent. of nitre, the protoplasm (primordial utricle) will undergo more and more contraction, until with a 10 per cent. solution, it will entirely separate from the cell wall, and appear as a much contracted vesicle lying freely in the cell cavity.

In repeating these experiments I have, however, found that in a very great number of cases—I dare not at present say in all—the contracted primordial utricle is connected to the cell wall by fine strings of protoplasm. The phenomenon is very distinctly shown by the cells of the main pulvinus of *Robinia pseudacacia*, in which I first made the discovery. Very frequently, instead of there being one main mass connected to the cell wall by the fine threads above-mentioned, the cell protoplasm becomes divided into two or three globular masses, all of which are united to each other, and to the cell wall. The connecting threads very generally exhibit nodal thickenings, each node presenting a most perfect spherical form.

Since the sections examined must be somewhat thick to avoid cutting into the cells, observations as to the relation of the threads with the pits present some difficulty. However, in several well-defined instances I have seen clearly that many threads do go to pits, and also that in two adjoining cells, many threads on different sides of the common cell wall are exactly opposite one another. When saturated salt solution is added, some of the threads may give way. Each free end immediately contracts: the one to the main mass, and the other to form a minute sphere lying on the side of the cell wall. I have succeeded fairly well in fixing and staining these plasmolytic figures.

The above plasmolytic phenomena are apparent in all the pulvini I have examined, *e.g.*, *Desmodium*, *Mimosa*, *Oxalis*, *Robinia*, *Amicia*, *Phoseolus*, *Apios*, &c. I have also observed it in stems and roots, notably in the *Beet*.

I should also mention here that I have succeeded in showing the passage of the protoplasm through the cell wall when the wall is left intact, and not swollen by reagents, the method consisting in treating thin sections of fresh material at once with saturated picric acid, washing with alcohol, and staining with aniline blue.

V. "Note on the Discovery of Bacilli in the Condensed Aqueous Vapour of the Breath of Persons affected with Phthisis." By ARTHUR RANSOME, M.A., M.D. Communicated by Dr. W. ROBERTS, F.R.S. Received November 8, 1882.

In the year 1869 I communicated to the Literary and Philosophical Society of Manchester a paper "On the Organic Matter of the Human Breath in Health and Disease" (Memoirs, vol. iv, 3rd Series, p. 234).

The method employed was to condense the vapour of the breath in a large glass globe, surrounded by ice and salt; and the fluid so collected was then examined chemically and microscopically. The vapour in condensing was found to carry with it all the organic matter contained in the breath. Certain chemical variations in this fluid were noted, and in addition to epithelial scales, which were also found in health, the breath of diseased persons was found to contain certain organised bodies.

It appeared probable that the breath of persons in advanced stages of phthisis would contain the bacillus of tubercle, and that this organism could be rendered visible by the method of staining.

The aqueous vapour of the breath of certain cases of advanced phthisis was accordingly condensed in above-mentioned manner, and each specimen was separately examined. In order to carry down the organic particles, and to afford a basis by which the substances obtained could be made to adhere to the microscopic cover-glasses, it was necessary to add some glutinous material to the condensed fluids. In some instances I used for this purpose fresh white of an egg, in others mucus from the mouth, that had been separately examined by staining, and which had been found free from bacilli. No attempt was made to sterilise any of the fluids, the ordinary bacteria of putrefaction being left unstained in the process used.

The method of staining employed was that suggested by Dr. Heneage Gibbes, in which magenta and aniline are first used, and then after discharging the colour, from all but the bacilli, by dilute nitric acid, chrysoidin is used to throw them into relief. (See "Brit. Med. Journal," August 5, 1882.) It is affirmed that by this method only the *Bacillus tuberculosus* is stained red.

I have now to state that in the aqueous vapour obtained from two persons suffering from phthisis, I have found specimens of a bacillus, which takes the staining in the same manner as the bacillus found in phthisical sputa and in tubercle, and which is indistinguishable from that organism. In several cases of acute phthisis the search for the organism was unsuccessful, and none were found in the aqueous vapour condensed from the waiting room of the Consumption Hospital in Manchester.

Koch has shown that the dust from dried phthisical sputa is capable of conveying the disease, but the above-mentioned discovery of the bacillus in the breath renders it probable that particles contagious to susceptible individuals are constantly being breathed in with the air, and it is possible that at some future time, the bacillus of tubercle may, by careful cultivation of the vapour of crowded rooms, be obtained from this source.

November 23, 1882.

THE PRESIDENT in the Chair.

In pursuance of the Statutes, notice was given from the Chair of the ensuing Anniversary Meeting, and the list of Officers and Council nominated for election was read, as follows:—

President.—William Spottiswoode, M.A., D.C.L., LL.D.

Treasurer.—John Evans, D.C.L., LL.D.

Secretaries.— Professor George Gabriel Stokes, M.A., D.C.L., LL.D.
Professor Michael Foster, M.A., M.D.

Foreign Secretary.—Professor Alexander William Williamson, Ph.D., LL.D.

Other Members of the Council.—Professor W. Grylls Adams, M.A., John Ball, M.A.; Thomas Lauder Brunton, M.D., Sc.D.; Professor Heinrich Debus, Ph.D.; Francis Galton, M.A.; Professor Olaus Henrici, Ph.D.; Professor Thomas Henry Huxley, LL.D.; Professor E. Ray Lankester, M.A.; Professor Joseph Lister, M.D.; Professor Joseph Prestwich, M.A.; Professor Osborne Reynolds, M.A.; Professor Henry Enfield Roscoe, B.A., LL.D.; Marquis of Salisbury, K.G., M.A.; Osbert Salvin, M.A.; Warington W. Smyth, M.A., F.G.S.; Edward James Stone, M.A.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Monthly Means of the Highest and Lowest Diurnal Temperatures of the Water of the Thames, and Comparison with the corresponding Temperatures of the Air at the Royal Observatory, Greenwich." By Sir GEORGE BIDDELL AIRY, K.C.B., F.R.S., late Astronomer Royal. Received September 15, 1882.

In presenting to the Royal Society a partial reduction of the thermometrical observations made in the water of the Thames during a period of thirty-five years, I commence with a brief history of the undertaking and progress of this work.

The observations were instituted at the suggestion of the conductors of the Medical Department in the Office of the Registrar-General of Births, Deaths, and Marriages, with the view of supplying some knowledge of an element which may possibly affect the sanitary condition of the Metropolis. The plan of observations was arranged at the Royal Observatory of Greenwich; and the instruments were procured and mounted, and repaired, when necessary, under the care successively of James Glaisher, Esq., and William Ellis, Esq., Superintendents of the Magnetical and Meteorological Department of the Observatory. The self-recording instruments were attached to the Hospital Ships successively anchored in the Thames, nearly opposite to Greenwich: and their records were read and registered by the medical officers of those ships, and these written registers were transmitted every week to the Royal Observatory. And I cannot too strongly express my sense of the care with which the observations were made, the fidelity with which they were recorded, and the order and regularity with which they were transmitted to the Royal Observatory. The weekly register, when received at the Observatory, was combined with the brief record of other meteorological facts observed at the Royal Observatory, and (with the medical record) was published every week by the Registrar-General.

I have been favoured by Mr. Ellis, who, at my request, has kindly superintended the preparation of the results of observations of thermometers in the water of the Thames, with the following remarks on the nature of the observations and the elements for their reduction.

"The observations, from the commencement of the series in May, 1844, until April, 1870, were taken at the 'Dreadnought' Hospital Ship, moored in the river a little above Greenwich. The thermometers were inclosed in an upright wooden trunk attached to the side of the ship, its lower portion projecting into the water; the trunk was closed at the bottom; the closing plate and that portion of the sides which was under water being perforated with holes, to allow the water easily to flow through. The thermometers were suspended in the trunk so as to be about 2 feet below the surface of the water and 1 foot above the bottom of the trunk.

"In 1870, April, the observations at the 'Dreadnought' terminated; in 1871, January, observations were recommenced at the police-ship 'Scorpion,' moored in Blackwall Reach (between Greenwich and Blackwall), the thermometers being mounted in the same way as previously at the 'Dreadnought'; in 1874, May, the wooden trunk and thermometers were removed from the 'Scorpion' to the police-ship 'Royalist,' moored in the same place. In 1879, October, the 'Royalist' was damaged by another vessel coming into collision with her; after repair, the 'Royalist' was not again moored in the river, but was placed on the river bank near high-water mark, in which position no further observations of the temperature of the water could be made. The observations have not, to the present time, been resumed in any other position.

"The instruments employed throughout were one for highest temperature and one for lowest temperature. For highest temperature two constructions have been successively used; the earlier, in which the mercury, with rising temperature, pushes up a steel index, leaving it detached when the temperature falls; the later, in which the column of mercury becomes divided on fall of temperature, the principal portion of the column being left in the tube. For lowest temperature a spirit thermometer was employed, its index being contained within the column of spirit. The index-errors of the two thermometers in use were properly determined, and corrections for them were applied when necessary.

"The thermometers were read every morning at 9 A.M."

In the first steps of publication of these observations in the Greenwich Meteorological Table, given in the Registrar-General's "Weekly Report of Births and Deaths in London," the printed readings, in early years, were referred to the civil day preceding that on which the instruments were actually read; in later years they were referred to the civil day on which the instruments were read.

The observations of atmospheric temperature at the Royal Observatory were made with the maximum and minimum thermometers in ordinary use, at the elevation 4 feet above the ground; readings adopted here are those which correspond to the civil day preceding that on which the Thames thermometers were read.

It will be remarked that the indications of the thermometers in the Thames were read only once in each day. I could have wished that a greater number of readings could have been taken, sufficiently numerous to exhibit the dependence of the temperature of the Thames water upon the phase of the tide. But under the circumstances this was impracticable. To establish a self-registering apparatus was out of the question; and if on a few occasions we had gone through the labour of making observations at every hour of day and night, the conclusions deduced from those few instances might have been vitiated by accidents. But I am able to assert positively, as a result from the reductions to be exhibited in the following pages, that nothing has been lost from the restriction of the plan of observation. It will be seen that the daily change of temperature, produced by the aggregate of strictly diurnal change (depending on the solar hour) and tidal change (depending on the moon's apparent position) is so small, that it is impossible to attach with any certainty a sensible value to either of these causes.

I now proceed to describe the principal steps in the reduction of the observations.

In the weekly publication of these observations by the Registrar-General, the weekly means of each observed element were also exhibited. In preparation for a detailed publication of the whole, I had the entire series of these weekly means collected, each being accompanied with notes of the principal phases of the moon, the occurrence of remarkable storms, &c., occurring within the week. (This *résumé* exists, and is available for any discussion which might be suggested; I propose to offer it for deposit at the Royal Observatory.) But on general examination of the collected means, I did not perceive that any result could be expected which would justify the labour and expense of printing the whole. For instance, if there were any remarkable dependence on the phase of tide, different values for the "excess of mean temperature of the water above mean temperature of the air," would occur in the weeks which included respectively new moon, first quarter, full moon, third quarter; and these would recur with little alteration for several months. But on general examination I do not see anything which would justify more technical discussion directed to this point. Finally, I decided on exhibiting only the means of deductions as to temperature for each calendar month, and omitting all other phenomena. As the succession of weeks and the succession of entire months do not generally coincide, the rule was established, to adopt the first entire week in each calendar month as the first of the weeks to be used, in conjunction with three or four weeks following, to form the monthly mean. Thus, some months contain four weeks and some contain five weeks. For instance, the month of 1846, March, contains the five weeks, March 1—7, 8—14, 15—21, 21—28, 28—April 4; but the next month contains only the four weeks, April 5—11, 12—18, 19—25, 26—May 2.

By this system, the results, as far as they appear to possess any value, are brought into the compass of five convenient tables of double entry, which, with their columnar and lateral means, appear to give all the information that can be desired. The contents of the several tables are:—

Table I.—Monthly Mean Temperature of the Water of the Thames.

Table II.—Monthly Mean Atmospheric Temperature at the Royal Observatory.

Table III.—Monthly Mean Excess of Thames Temperature above Observatory Atmospheric Temperature.

Table IV.—Monthly Mean of Diurnal Range of Temperature of the Water of the Thames.

Table V.—Monthly Mean of Diurnal Range of Atmospheric Temperature at the Royal Observatory.

And the following appear to be the legitimate epitomized inferences:

(1.) The mean temperature of the Thames water is higher than that of the Observatory thermometers by 1°·5. But the locality of the Observatory thermometer is, in hypsometrical elevation, about 160 feet above that of the Thames thermometers. It would seem probable, therefore, that the mean temperature of the water is higher than the climatic temperature by only a small fraction of a degree.

(2.) This difference is not uniform through the year. With some irregularities, the greatest excess of Thames temperature occurs in October and the least in February. But the autumnal difference exceeds the spring difference by only 1°·8. It seems not improbable that this is the effect of a slight communication with the sea, whose surface-waters have accumulated in autumn the effect of solar radiation through the summer; with contrary effect at the opposite season.

(3.) The mean range of temperature through the day is 2°·1, and this expresses the numerical change from the lowest solar temperature, or the lowest temperature in the first tide, or the lowest temperature in the second tide (whichever may be the lowest), to the highest solar temperature, or the highest temperature in the first tide, or the highest temperature in the second tide (whichever may be the highest). It is evident that the change of temperature due to the diurnal change of solar action, and the change of temperature due to each of the tides, must each, individually, be very small.

(4.) It appears to me that the fundamental inference must be this: that the material water is very little changed at Greenwich by the tide. Although a vast body of water rushes up at every flow, running with great speed, and sometimes raising the surface by 20 feet, yet nearly the same water runs down at ebb, and is again brought up, with all its contents, at the next flow. These expressions are to be taken as modified by the descent of fresh water from the land; but the amount of that water must be small in comparison with the mass which it joins in the Thames at London.

(5.) I do not imagine that the tidal action has any beneficial effect on the climate of London, except that probably the agitation of the water produces mechanical agitation of the air, and thus destroys injurious stagnation.

Table I.—Monthly Mean Temperature of the Water of the Thames at Greenwich.

Year.	Month.												Sums.	Means.
	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.		
	°	°	°	°	°	°	°	°	°	°	°	°	°	°
1844														
1845	39·3	34·4	38·8	49·6	52·9	63·0	63·6	60·1	56·8	50·9	46·1	40·6	596·1	49·7
1846	43·3	43·9	47·4	50·9	60·4	73·1	67·1	67·1	65·9	51·9	44·4	36·9	652·3	54·4
1847	36·5	38·2	42·9	47·1	59·4	63·2	68·9	64·6	56·2	52·8	46·5	41·4	617·7	51·5
1848	35·5	42·7	46·3	51·7	64·2	63·1	65·5	62·1	59·4	52·3	42·9	41·0	626·7	52·2
1849	41·8	43·5	45·2	47·9	58·2	64·1	67·5	63·9	59·3	50·1	44·8	37·3	623·6	52·0
1850	33·0	41·6	41·5	49·4	54·0	63·8	64·8	63·3	57·5	48·4	46·3	39·6	603·2	50·3
1851	41·0	40·1	42·8	49·7	54·3	61·9	65·0	65·7	58·9	54·6	41·3	41·5	616·8	51·4
1852	40·9	41·0	42·2	48·3	55·5	61·2	70·6	66·2	60·1	48·6	46·7	46·3	627·6	52·3
1853	40·0	37·1	42·4	48·6	55·5	62·6	63·4	62·3	58·1	53·5	43·5	38·0	605·0	50·4
1854	39·1	41·3	46·2	52·1	56·1	59·4	64·8	63·8	62·9	53·6	44·7	41·8	625·8	52·1
1855	39·9	34·6	41·4	49·1	53·2	59·7	66·4	66·1	60·9	54·1	44·2	38·5	608·1	50·7
1856	40·5	42·4	43·5	50·7	54·0	63·0	65·4	66·6	58·2	54·5	44·1	42·6	625·5	52·1
1857														
1858	39·2	37·8	41·7	50·0	56·5	67·9	65·1	65·4	62·1	56·7	42·1	41·9	626·4	52·2
1859	41·4	44·8	47·9	49·7	55·3	65·4	71·1	67·2	61·2	60·2	44·7	38·8	647·7	54·0
1860	40·8	36·9	42·9	47·0	56·9	58·8	62·5	60·8	57·6	51·1	45·3	39·8	600·4	50·0
1861	35·0	43·1	44·9	49·4	56·5	63·4	65·3	65·7	61·1	57·0	43·7	41·4	626·5	52·2
1862	39·7	43·4	46·4	51·0	58·3	60·2	62·5	63·4	60·0	54·7	43·6	42·7	625·9	52·2
1863	41·2	43·2	45·6	53·1	56·5	63·1	66·2	65·3	60·2	54·8	48·1	43·5	640·8	53·4
1864	39·8	37·5	43·7	50·5	58·8	61·7	66·0	63·2	60·5	52·9	44·4	42·5	621·5	51·8
1865	38·5	38·2	39·6	53·9	60·6	65·7	65·7	64·3	65·9	54·0	43·9	43·1	633·4	52·8
1866	42·2	39·3	41·8	50·6	53·7	62·9	65·2	61·5	58·2	53·7	44·7	41·5	615·3	51·3
1867	35·1	44·2	40·5	49·9	55·2	62·0	62·7	63·7	59·6	49·7	43·5	36·8	602·9	50·2
1868	37·6	41·5	45·9	50·0	59·6	64·5	68·3	65·1	60·2	50·4	43·3	43·8	630·2	52·5
1869	41·6	44·4	40·4	51·1	53·9	57·2	65·1	62·4	57·9	51·1	42·8	37·9	605·8	50·5
1870														
1871	35·9	41·8	45·6	49·6	55·1	58·8	63·8	66·6	61·1	51·9	41·3	37·7	609·2	50·8
1872	40·2	44·5	47·0	51·7	54·7	62·6	68·1	65·1	60·5	50·3	45·8	42·0	632·5	52·7
1873	41·9	36·6	43·5	49·2	53·9	61·3	66·4	65·1	57·7	52·7	44·4	42·4	615·1	51·3
1874	41·1	40·3	44·3	51·3	56·1	62·7	67·5	63·0	60·0	54·4	49·3	34·4	624·4	52·0
1875	41·2	38·1	43·1	48·9	59·0	62·8	62·7	66·1	63·2	51·2	42·5	38·9	617·7	51·5
1876	39·4	42·1	42·2	50·1	54·1	61·9	67·9	66·9	59·2	55·5	46·0	43·2	628·5	52·4
1877	42·9	44·3	42·6	49·2	53·5	64·0	65·0	65·8	57·8	51·6	47·1	41·4	625·2	52·1
1878	41·1	42·5	45·4	51·0	59·4	62·5	67·7	66·1	60·2	54·7	42·2	36·5	629·3	52·4
1879	33·9	38·8	43·3	47·2	52·3	59·8	60·7	62·3	58·2	53·0	39·3	35·9	584·7	48·7
Sums	1300·5	1344·1	1438·9	1649·5	1857·6	2067·3	2168·5	2126·8	1976·6	1746·9	1463.5	1331·6	20471·8	
Means	39·4	40·7	43·6	50·0	56·3	62·6	65·7	64·4	59·9	52·9	44·3	40·4	...	51·7

During the years 1844, 1857, and 1870 the observations were incomplete, the results for those years have in consequence been altogether omitted. The monthly values *in different figures* are estimations, inserted in order to complete the numbers for the particular years in which they occur, for the purpose of taking means.

Table II.—Monthly Mean Atmospheric Temperature at the Royal Observatory, Greenwich.

Year.	Month.												Sums.	Means.
	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.		
	°	°	°	°	°	°	°	°	°	°	°	°	°	
1844														
1845	38·2	32·8	37·0	48·6	49·4	61·0	59·8	57·3	53·9	48·7	45·0	41·1	572·8	47·7
1846	44·7	44·1	44·3	47·7	56·7	68·3	68·0	63·4	63·3	49·9	41·8	36·1	628·3	52·4
1847	35·7	36·0	42·5	46·5	58·1	57·8	66·6	61·2	54·9	52·6	46·0	41·2	599·1	49·9
1848	35·8	44·0	45·3	49·1	58·6	60·2	62·4	59·2	56·9	50·6	43·4	41·2	606·7	50·6
1849	42·5	43·3	43·2	46·3	55·9	58·6	63·6	64·7	58·5	51·5	43·3	38·1	609·5	50·8
1850	34·6	44·6	42·1	49·7	53·6	61·7	63·6	61·0	56·3	47·5	46·1	41·4	602·2	50·2
1851	42·3	40·7	43·9	45·6	52·0	59·4	62·0	63·1	56·1	52·6	38·3	40·2	596·2	49·7
1852	42·9	40·7	42·5	46·8	53·0	59·7	69·5	63·7	56·7	50·2	47·5	48·1	621·3	51·8
1853	37·7	34·1	40·3	46·7	53·0	60·2	61·4	60·3	56·1	52·7	40·5	33·7	576·7	48·1
1854	39·3	40·7	46·3	50·2	53·6	58·3	61·9	63·3	59·5	50·6	40·3	42·6	606·6	50·5
1855	32·1	29·0	38·4	47·1	51·6	58·9	63·7	63·2	57·9	49·8	42·0	36·5	570·2	47·5
1856	38·5	43·1	41·0	46·9	51·8	60·1	64·1	63·5	57·1	51·9	40·0	41·8	599·8	50·0
1857														
1858	37·8	34·5	44·2	48·4	55·4	65·7	63·3	63·7	61·3	52·0	39·8	40·3	606·4	50·5
1859	40·4	46·4	46·2	48 5	55·2	63·3	69·0	64·8	58·1	56·6	40·3	37·0	625·8	52·2
1860	39·4	37·0	42·3	45·8	55·6	56·8	60·6	59·2	54·3	50·4	40·8	37·7	579·9	48·3
1861	35·5	44·1	45·0	45·9	57·0	61·2	63·2	64 8	59·4	54·1	40·7	40·9	611·8	51·0
1862	37·9	41·2	45·3	50·1	57·4	58·3	61·5	60·4	58·9	52·3	40·8	42·8	606·9	50·6
1863	41·7	42 7	44·7	51·1	55·1	60·4	61·8	63·0	54·1	52·2	45·5	41·7	614·0	51·2
1864	37·2	36·7	42·3	48·1	54·7	60·2	65·8	59·5	57·8	51·6	43·4	40·8	598·1	49·8
1865	37·1	37·1	37·4	54·6	57·2	62·0	63·8	62·5	65·0	51·3	45·7	42·4	616·1	51·3
1866	41·9	39·3	42·5	50·1	53·2	62·7	62·6	60·7	57·9	50·7	42·9	39·7	604·2	50·4
1867	37·1	44·4	41·2	50·9	55·1	60·2	59·8	64·3	58·3	50·0	41·2	37·9	600·4	50·0
1868	38·8	43·2	45·4	50·1	58·7	64·5	69·3	65·3	59·3	48·3	42·5	44·9	630·3	52·5
1869	42·7	43·9	39·2	52·7	53·1	57·0	66·3	61·7	61·0	48·9	40·6	38·8	605·9	50·5
1870														
1871	33·9	44·1	45·8	50·0	53·7	57·8	63·3	67·5	58·3	49·9	36·5	39·2	600·0	50·0
1872	41·8	46·0	45·1	51·5	52·1	61·8	66·1	62·8	58·0	48·2	45·5	43·2	622·1	51·8
1873	41·5	35·3	44·3	48·2	52·1	61·0	65·5	63·2	56·2	46·9	44·2	40·1	598·5	49·9
1874	41·6	39·2	45·0	51·6	54·5	59·5	66·4	61·5	58·6	52·4	45·3	30·6	606·2	50·5
1875	42·7	35·3	42·6	47·6	57·5	60·4	60·0	64·0	60·7	48·7	39·4	39·9	598·8	49·9
1876	37·3	42·4	41·6	48·2	52·2	60·5	67·1	64·9	57·1	52·3	44·6	43·9	612·1	51·0
1877	42·0	43·5	41·5	46·2	52·4	63·2	62·2	63·6	53·3	48·6	45·2	41·0	602·7	50·2
1878	39·3	43·4	42·0	52·1	56·0	61·2	64·7	64·3	57·6	50·6	39·5	33·5	604·2	50·3
1879	30·9	38·8	42·6	44·0	49·9	58·2	60·2	60·6	56·8	48·8	36·3	34·9	562·0	46·8
Sums	1282·8	1331·6	1413·0	1606·9	1795·4	2000·1	2109·1	2066·2	1909·2	1673·4	1394·9	1313·2	19895·8	
Means	38·9	40·4	42·8	48·7	54·4	60·6	63·9	62·6	57·9	50·7	42·3	39·8	...	50·2

The results for the years 1844, 1857, and 1870 have been omitted, in order to render the table entirely comparative with that giving the Thames temperatures.

Table III.—Monthly Mean Excess of Thames Temperature at Greenwich, over Atmospheric Temperature at Royal Observatory, Greenwich.

Year.	Month.												Sums.	Means.
	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.		
	°	°	°	°	°	°	°	°	°	°	°	°	°	°
1844														
1845	+ 1·1	+ 1·6	+ 1·8	+ 1·0	+ 3·5	+ 2·0	+ 3·8	+ 2·8	+ 2·9	+ 2·2	+ 1·1	− 0·5	+ 23·3	+1·9
1846	− 1·4	− 0·2	+ 3·1	+ 3·2	+ 3·7	+ 4·8	− 0·9	+ 3·7	+ 2·6	+ 2·0	+ 2·6	+ 0·8	+ 24·0	+2·0
1847	+ 0·8	+ 2·2	+ 0·4	+ 0·6	+ 1·3	+ 5·4	+ 2·3	+ 3·4	+ 1·3	+ 0·2	+ 0·5	+ 0·2	+ 18·6	+1·6
1848	− 0·3	− 1·3	+ 1·0	+ 2·6	+ 5·6	+ 2·9	+ 3·1	+ 2·9	+ 2·5	+ 1·7	− 0·5	− 0·2	+ 20·0	+1·7
1849	− 0·7	+ 0·2	+ 2·0	+ 1·6	+ 2·3	+ 5·5	+ 3·9	− 0·8	+ 0·8	− 1·4	+ 1·5	− 0·8	+ 14·1	+1·2
1850	− 1·6	− 3·0	− 0·6	− 0·3	+ 0·4	+ 2·1	+ 1·2	+ 2·3	+ 1·2	+ 0·9	+ 0·2	− 1·8	+ 1·0	+0·1
1851	− 1·3	− 0·6	− 1·1	+ 4·1	+ 2·3	+ 2·5	+ 3·0	+ 2·6	+ 2·8	+ 2·0	+ 3·0	+ 1·3	+ 20·6	+1·7
1852	− 2·0	+ 0·3	− 0·3	+ 1·5	+ 2·5	+ 1·5	+ 1·1	+ 2·5	+ 3·4	− 1·6	− 0·8	− 1·8	+ 6·3	+0·5
1853	+ 2·3	+ 3·0	+ 2·1	+ 1·9	+ 2·5	+ 2·4	+ 2·0	+ 2·0	+ 2·0	+ 0·8	+ 3·0	+ 4·3	+ 28·3	+2·4
1854	− 0·2	+ 0·6	− 0·1	+ 1·9	+ 2·5	+ 1·1	+ 2·9	+ 0·5	+ 3·4	+ 3·0	+ 4·4	− 0·8	+ 19·2	+1·6
1855	+ 7·8	+ 5·6	+ 3·0	+ 2·0	+ 1·6	+ 0·8	+ 2·7	+ 2·9	+ 3·0	+ 4·3	+ 2·2	+ 2·0	+ 37·9	+3·2
1856	+ 2·0	− 0·7	+ 2·5	+ 3·8	+ 2·2	+ 2·9	+ 1·3	+ 3·1	+ 1·1	+ 2·6	+ 4·1	+ 0·8	+ 25·7	+2·1
1857														
1858	+ 1·4	+ 3·3	− 2·5	+ 1·6	+ 1·1	+ 2·2	+ 1·8	+ 1·7	+ 0·8	+ 4·7	+ 2·3	+ 1·6	+ 20·0	+1·7
1859	+ 1·0	− 1·6	+ 1·7	+ 1·2	+ 0·1	+ 2·1	+ 2·1	+ 2·4	+ 3·1	+ 3·6	+ 4·4	+ 1·8	+ 21·9	+1·8
1860	+ 1·4	− 0·1	+ 0·6	+ 1·2	+ 1·3	+ 2·0	+ 1·9	+ 1·6	+ 3·3	+ 0·7	+ 4·5	+ 2·1	+ 20·5	+1·7
1861	− 0·5	− 1·0	− 0·1	+ 3·5	− 0·5	+ 2·2	+ 2·1	+ 0·9	+ 1·7	+ 2·9	+ 3·0	+ 0·5	+ 14·7	+1·2
1862	+ 1·8	+ 2·2	+ 1·1	+ 0·9	+ 0·9	+ 1·9	+ 1·0	+ 3·0	+ 1·1	+ 2·4	+ 2·8	− 0·1	+ 19·0	+1·6
1863	− 0·5	+ 0·5	+ 0·9	+ 2·0	+ 1·4	+ 2·7	+ 4·4	+ 2·3	+ 6·1	+ 2·6	+ 2·6	+ 1·8	+ 26·8	+2·2
1864	+ 2·6	+ 0·8	+ 1·4	+ 2·4	+ 4·1	+ 1·5	+ 0·2	+ 3·7	+ 2·7	+ 1·3	+ 1·0	+ 1·7	+ 23·4	+2·0
1865	+ 1·4	+ 1·1	+ 2·2	− 0·7	+ 3·4	+ 3·7	+ 1·9	+ 1·8	+ 0·9	+ 2·7	− 1·8	+ 0·7	+ 17·3	+1·4
1866	+ 0·3	0·0	− 0·7	+ 0·5	+ 0·5	+ 0·2	+ 2·6	+ 0·8	+ 0·3	+ 3·0	+ 1·8	+ 1·8	+ 11·1	+0·9
1867	− 2·0	− 0·2	− 0·7	− 1·0	+ 0·1	+ 1·8	+ 2·9	− 0·6	+ 1·3	− 0·3	+ 2·3	− 1·1	+ 2·5	+0·2
1868	− 1·2	− 1·7	+ 0·5	− 0·1	+ 0·9	0·0	− 1·0	− 0·2	+ 0·9	+ 2·1	+ 0·8	− 1·1	− 0·1	0·0
1869	− 1·1	+ 0·5	+ 1·2	− 1·6	+ 0·8	+ 0·2	− 1·2	+ 0·7	− 3·1	+ 2·2	+ 2·2	− 0·9	− 0·1	0·0
1870														
1871	+ 2·0	− 2·3	− 0·2	− 0·4	+ 1·4	+ 1·0	+ 0·5	− 0·9	+ 2·8	+ 2·0	+ 4·8	− 1·5	+ 9·2	+0·8
1872	− 1·6	− 1·5	+ 1·9	+ 0·2	+ 2·6	+ 0·8	+ 2·0	+ 2·3	+ 2·5	+ 2·1	+ 0·3	− 1·2	+ 10·4	+0·9
1873	+ 0·4	+ 1·3	− 0·8	+ 1·0	+ 1·8	+ 0·3	+ 0·9	+ 1·9	+ 1·5	+ 5·8	+ 0·2	+ 2·3	+ 16·6	+1·4
1874	− 0·5	+ 1·1	− 0·7	− 0·3	+ 1·6	+ 3·2	+ 1·1	+ 1·5	+ 1·4	+ 2·0	+ 4·0	+ 3·8	+ 18·2	+1·5
1875	− 1·5	+ 2·8	+ 0·5	+ 1·3	+ 1·5	+ 2·4	+ 2·7	+ 2·1	+ 2·5	+ 2·5	+ 3·1	− 1·0	+ 18·9	+1·6
1876	+ 2·1	− 0·3	+ 0·6	+ 1·9	+ 1·9	+ 1·4	+ 0·8	+ 2·0	+ 2·1	+ 3·2	+ 1·4	− 0·7	+ 16·4	+1·4
1877	+ 0·9	+ 0·8	+ 1·1	+ 3·0	+ 1·1	+ 0·8	+ 2·8	+ 2·2	+ 4·5	+ 3·0	+ 1·9	+ 0·4	+ 22·5	+1·9
1878	+ 1·8	− 0·9	+ 3·4	− 1·1	+ 3·4	+ 1·3	+ 3·0	+ 1·8	+ 2·6	+ 4·1	+ 2·7	+ 3·0	+ 25·1	+2·1
1879	+ 3·0	0·0	+ 0·7	+ 3·2	+ 2·4	+ 1·6	+ 0·5	+ 1·7	+ 1·4	+ 4·2	+ 3·0	+ 1·0	+ 22·7	+1·9
Sums.	+17·7	+12·5	+25·9	+42·6	+62·2	+67·2	+59·4	+60·6	+67·4	+73·5	+68·6	+18·4	+576·0	
Means	+0·5	+0·4	+0·8	+1·3	+1·9	+2·0	+1·8	+1·8	+2·0	+2·2	+2·1	+0·6	...	+1·5

During the years 1844, 1857, and 1870 the observations of Thames temperature were incomplete, the results for those years have in consequence been altogether omitted. The monthly values in *different figures* are estimations, inserted in order to complete the numbers for the particular years in which they occur for the purpose of taking means.

Table IV.—Monthly Mean of the Diurnal Range of the Temperature of the Water of the Thames at Greenwich.

Year.	Month.												Sums.	Means.
	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.		
	°	°	°	°	°	°	°	°	°	°	°	°	°	°
1844														
1845	0·8	0·4	1·0	1·0	0·5	0·8	0·3	0·4	0·4	0·4	1·1	2·1	9·2	0·8
1846	2·2	2·7	1·9	2·1	2·8	1·3	1·3	1·7	1·5	1·5	2·2	2·6	23·8	2·0
1847	1·6	1·5	0·6	0·7	2·1	3 6	4·1	1·6	0·4	0·4	0·7	1·1	18·4	1·5
1848	1·0	1·5	1·5	1·1	1·3	0·9	1·0	1·1	1·0	2·9	2·5	3·4	19·2	1·6
1849	1·6	1·6	1·5	3·7	3·0	2·3	2·2	2·1	2·8	3·2	3·1	3·5	30·6	2·5
1850	2·7	3·1	3·3	3·3	4·2	4·0	3·5	3·4	3·1	3·3	3·5	3·4	40·8	3·4
1851	3·6	3·7	3·5	3·5	4·4	3·8	2·0	2·1	2·3	2·0	2·7	2·7	36·3	3·0
1852	2·3	2·7	2·0	2·0	2·0	2·6	2·7	3·3	3·0	1·0	1·7	1·5	26·8	2·2
1853	0·6	0·9	2·4	1·8	2·5	2·1	2·2	2·5	2·0	1·7	1·5	1·3	21·5	1·8
1854	1·0	1·8	2·7	3·4	3·4	3·4	3·1	3·2	3·2	2·9	2·0	1·5	31·6	2·6
1855	2·7	1·5	2·0	2·5	2·6	3·4	2·9	2·1	3·0	2·7	3·2	2·7	31·3	2·6
1856	1·7	1·9	1·2	3·0	3·0	3·6	2·8	2·6	2·6	2·6	1·5	1·9	28·4	2·4
1857														
1858	0·7	0·8	1·0	3·2	3·5	2·2	3·0	2·8	1·8	1·3	0·2	0·0	20·5	1·7
1859	0·0	0·3	0·5	0·8	1·7	1·9	1·9	1·3	1·3	1·1	0·1	0·0	10·9	0·9
1860	0·9	1·8	1·8	1·1	1·6	1·0	1·0	1·0	1·0	1·0	1·0	1·0	14·2	1·2
1861	1·0	1·0	1·0	1·0	1·0	1·0	1·0	1·0	1·5	2·0	5·9	3·9	21·3	1·8
1862	3·0	2·3	2·8	2·2	2·2	2·2	2·1	2·2	2·2	2·3	2·3	3·0	28·8	2·4
1863	2·7	2·5	2·5	2·1	2·2	2·2	2·2	2·2	2·2	2·2	2·2	2·2	27·4	2·3
1864	3·2	3·2	2·2	3·1	2·5	1·9	2·0	1·9	2·2	2·2	1·3	1·3	27·0	2·3
1865	1·6	1·7	1·0	1·0	1·2	1·0	0·5	0·3	0·5	0·3	1·8	1·3	12·2	1·0
1866	2·2	2·0	1·0	1·2	1·1	1·2	0·9	0·8	0·7	1·9	2·2	2·3	17·5	1·5
1867	2·4	2·4	2·8	2·6	2·1	1·8	2·0	1·9	1·8	2·2	2·2	2·0	26·2	2·2
1868	1·6	1·3	1·6	1·8	1·5	1·4	0·5	1·3	1·1	1·5	1·6	1·8	17·0	1·4
1869	1·7	1·8	1·5	2·3	2·1	1·9	2·2	1·6	1·8	1·4	1·4	2·0	21·7	1·8
1870														
1871	3·0	3·5	3·1	3·8	4·1	3·1	3·5	3·0	3·6	2·7	3·3	2·7	39·4	3·3
1872	2·2	2·0	2·7	2·8	2·2	2·8	2·9	2·5	2·3	2·3	2·4	2·1	29·2	2·4
1873	2·1	2·0	2·4	2·7	3·1	2·5	2·1	1·9	1·9	2·4	3·0	2·3	28·4	2·4
1874	2·3	2·4	2·7	3·1	2·3	1·8	1·8	1·9	1·8	1·8	1·9	2·1	25·9	2·2
1875	2·6	2·0	1·9	2·2	2·4	1·8	1·7	1·6	1·7	2·0	1·8	1·8	23·5	2·0
1876	2·2	1·8	1·8	2·7	2·7	2·8	2·5	2·5	2·1	2·4	2·4	2·1	28·0	2·3
1877	2·4	1·9	2·3	2·2	2·7	2·5	2·1	1·9	1·7	2·0	2·0	2·1	25·8	2·2
1878	2·3	2·1	2·3	2·6	2·3	2·3	2·5	2·5	2·6	2·6	2·3	2·5	28·9	2·4
1879	1·8	2·3	2·3	2·7	3·2	3·1	2·6	2·8	2·1	2·9	2·5	2·5	30·8	2·6
Sums	63·7	64·4	64·8	75·3	79·5	74·2	69·1	65·0	63·2	65·1	69·5	68·7	822·5	
Means	1·9	2·0	2·0	2·3	2·4	2·2	2·1	2·0	1·9	2·0	2·1	2·1	...	2·1

During the years 1844, 1857, and 1870 the observations were incomplete, the results for those years have, in consequence, been altogether omitted. The monthly values in *different figures* are estimations, inserted in order to complete the numbers for the particular years in which they occur, for the purpose of taking means.

Table V.—Monthly Mean of the Diurnal Range of Atmospheric Temperature at the Royal Observatory, Greenwich.

Year.	Month.												Sums.	Means.
	January.	February.	March.	April.	May.	June.	July.	August.	September.	October.	November.	December.		
	°	°	°	°	°	°	°	°	°	°	°	°	°	°
1844														
1845	7·2	9·4	15·1	15·8	13·9	18·0	14·1	14·5	16·9	13·6	10·7	10·0	159·2	13·3
1846	7·0	7·8	12·3	13·2	19·0	23·3	20·8	16·5	19·0	9·6	7·3	6·3	162·1	13·5
1847	7·6	10·1	15·9	15·3	19·2	14·4	21·5	17·7	15·6	12·0	11·0	6·8	167·1	13·9
1848	8·1	10·9	15·0	19·3	27·0	18·2	20·6	17·1	20·1	14·1	13·4	9·9	193·7	16·1
1849	11·6	14·5	13·2	16·9	15·4	20·6	24·5	19·7	16·6	15·4	12·5	9·2	190·1	15·8
1850	8·8	11·4	16·2	17·3	18·9	23·2	19·5	18·9	17·0	14·0	11·4	8·5	185·1	15·4
1851	10·3	13·4	12·3	16·7	20·3	21·6	17·9	19·8	20·2	13·1	11·3	8·1	185·0	15·4
1852	11·7	13·3	16·3	23·6	18·9	18·4	25·5	18·6	16·5	14·6	9·7	9·2	196·3	16·4
1853	8·7	10·6	16·3	14·1	20·0	19·0	16·1	18·1	18·6	14·7	12·4	8·9	177·5	14·8
1854	11·2	15·5	19·5	22·4	20·1	19·0	20·6	22·8	25·3	17·4	12·1	10·2	216·1	18·0
1855	8·1	10·0	14·5	21·6	17·1	20·8	19·1	19·5	19·8	12·5	9·3	9·8	182·1	15·2
1856	8·6	10·0	13·8	18·5	16·6	20·8	20·8	18·9	15·9	13·9	12·3	9·1	179·2	14·9
1857														
1858	11·6	11·5	18·6	19·7	21·5	25·1	22·5	22·8	18·8	15·6	11·7	8·2	207·6	17·3
1859	10·5	15·7	13·7	17·7	19·6	21·0	24·4	21·1	17·9	13·9	13·3	10·0	198·8	16·6
1860	9·8	14·1	13·9	19·1	19·9	16·5	19·0	15·6	18·2	14·1	10·6	7·9	178·7	14·9
1861	11·0	11·1	16·1	18·7	22·0	19·7	18·9	21·8	19·5	15·8	13·2	10·5	198·3	16·5
1862	9·4	10·1	11·7	16·8	18·5	18·1	20·5	19·1	17·0	14·4	11·1	9·5	176·2	14·7
1863	10·5	13·8	19·0	20·8	21·9	19·1	25·3	19·5	17·0	13·0	11·3	10·9	202·1	16·8
1864	9·9	11·1	15·8	16·1	19·5	20·8	25·6	23·5	17·8	14·2	12·1	9·2	195·6	16·3
1865	8·6	10·6	12·7	24·9	20·8	24·1	21·3	19·5	23·0	16·9	13·8	9·1	205·3	17·1
1866	11·2	12·5	14·0	18·5	21·0	21·2	19·8	17·0	13·1	13·5	12·2	11·3	185·3	15·4
1867	10·1	11·0	12·7	16·6	20·2	20·7	19·9	20·3	17·7	15·2	11·8	10·0	186·2	15·5
1868	9·0	13·1	16·8	18·5	24·3	25·3	27·3	21·0	18·2	17·2	10·4	9·7	210·8	17·6
1869	10·2	12·3	12·6	19·9	17·7	20·7	23·2	20·1	15·2	15·1	11·0	8·7	186·7	15·6
1870														
1871	8·0	13·2	17·2	17·8	22·2	18·1	19·6	25·4	16·8	15·5	10·9	8·3	193·0	16·1
1872	9·3	12·6	15·9	20·9	19·2	21·9	22·5	20·7	18·9	15·1	9·6	8·4	195·0	16·2
1873	8·6	8·7	16·6	19·7	19·7	19·0	23·5	19·6	19·4	16·1	10·8	10·7	192·4	16·0
1874	11·2	11·5	15·9	21·4	23·4	21·5	25·4	21·7	18·0	13·5	12·7	7·9	204·1	17·0
1875	9·4	9·2	13·9	20·4	22·7	20·0	17·8	20·0	18·5	12·7	9·7	8·9	183·2	15·3
1876	11·3	10·6	14·5	19·5	22·1	21·7	24·9	23·4	16·8	12·8	10·1	7·7	195·4	16·3
1877	11·7	11·2	14·0	15·2	17·0	25·5	20·5	19·0	19·3	16·8	12·1	9·6	191·9	16·0
1878	9·7	9·6	14·7	17·5	17·5	20·4	19·7	17·3	18·6	12·7	9·1	8·9	175·7	14·6
1879	6·2	8·7	14·9	16·1	18·2	17·2	16·0	17·1	15·0	12·8	10·6	9·9	162·7	13·6
Sums	316·1	379·1	495·6	610·5	655·3	674·9	698·6	647·6	596·2	471·8	371·5	301·3	6218·5	
Means	9·6	11·5	15·0	18·5	19·9	20·5	21·2	19·6	18·1	14·3	11·3	9·1	...	15·7

The results for the years 1844, 1857, and 1870, have been omitted, in order to render the table entirely comparative with that giving ranges of Thames temperature.

II. "Experimental Determinations of Magnetic Susceptibility and of Maximum Magnetisation in Absolute Measure." By R. Shida, Thomson Experimental Scholar, University Glasgow. Communicated by Sir William Thomson, F.R.S. Received October 10, 1882.

(Abstract.)

This paper contains the results of a series of experimental determinations of the magnetisation, magnetic susceptibility, &c., of different specimens of iron and steel, in centimetre gramme second units, by means of the direct magnetometric method shown to me by Sir William Thomson, as founded upon a method originated by Coulomb and mathematically discussed by Green.

A number of thin wires (from No. 20 to 22 B.W.G.) of soft iron and steel were tried in the first elaborate series of investigations. The experiments were varied by varying the strength of the magnetising force through a wide range; and for each magnetising force, and for each wire, the experiment was commenced by subjecting the wire to the application and removal of a longitudinal stress a certain number of times in succession (that is, "ons and offs"), while the magnetisation and magnetic susceptibility of the wire were determined for each degree of magnetising force, and both while the wire was actually under the influence of a constant pull (a case to be denoted by "on"), and while it was free from a pull (a case to be denoted by "off"). In the case of soft iron wires, the effects of suddenly reversing the magnetising force, and of "ons and offs" after the reversal of the force, were also investigated. Some interesting and remarkable results followed from these experiments; and the evaluations, made from these results, of the intensity of magnetisation and magnetic susceptibility, are carefully tabulated, and also represented graphically, for the sake of comparison, in two sets of curves—one, which shows the intensity of magnetisation, that is to say, in which the abscissæ are proportional to the magnetising force and the ordinates to the intensity of magnetisation; and the other, which shows the magnetic susceptibility, that is to say, in which the abscissæ and ordinates are respectively proportional to the force and the susceptibility.

The curves of the intensity of magnetisation show that the effects of "ons and offs," in augmenting the magnetisation of soft iron wires, are astonishingly great for low magnetising forces, and that, as the latter is gradually increased, the wires seem to lose their retentiveness gradually, so much so, in point of fact, that when the magnetising force exceeds a certain value (60 c.g.s. or so) the operation of "ons and offs" produces no permanent magnetisa-

tional effect; whereas, for a magnetising force below that value, the simple reversal of that force is not so effective as to annul the permanent effects of "ons and offs," or even to reverse the magnetic polarities of the wires. But an equally, if not more, remarkable result is found in the fact that the intensity of magnetisation of soft iron wires is greater or less while it is pulled than while it is unpulled, according as the magnetising force is below or above a certain critical value—a result which confirms that given in Sir William Thomson's paper on the "Electrodynamic Qualities of Metals," Part VII. It is quite evident, however, that this critical value is different, not only for different kinds of soft iron wire, but for different amounts of the pull to which the wire is subjected. The singularity of the existence of a critical point in a soft iron wire is only intensified by the fact that, whilst the permanent magnetisational effects of "ons and offs" on a wire of soft iron and of steel (pianoforte wire at least) are similar in kind, there is found no such thing as a critical point in the latter, in which the magnetisation is greater in the case of "off" than in the case of "on" for every degree of magnetising force.

For high magnetising forces the curves of the intensity of magnetisation all become asymptotes parallel to the line of abscissæ, proving that there is a limit to the magnetisability of iron and steel, as was first shown by Joule. In the case of "off," the maximum intensity of magnetisation is found to be approximately 1420, both in the soft iron wires and in the steel pianoforte wire, and in the cases of "on," it is more or less below that value, the minimum magnetising force corresponding to that magnetisation being in each case roughly 80 units; while in the glass-hard-tempered steel wire, to which no weight was applied at all, the maximum magnetisation is found to be slightly lower.

The steepness of the commencement of the curves of magnetic susceptibility in the case of the soft iron wires is striking, owing to retentiveness; indeed, the magnetic susceptibility of these wires varies through a vast range. Taking, for example, the case of the soft iron wire of No. 22 B.W.G., in which the weight used for "ons and offs" is 8 kilos., the susceptibility for the magnetising force (about ·545) of the earth's vertical component; is roughly 730 for "on" (8 kilos.) and 330 for "off," it is about 65 at the critical point (about 15), while it is only about 17 and 17½ in the cases of "on" and "off" respectively for the minimum magnetising force (about 80) corresponding to the maximum magnetisation. On the other hand, the susceptibility-curves for the steel wires are neither so steep nor so regular as those for the iron wires, but have a few maxima and minima; it is, however, all but certain that by using a heavier weight than the one actually used for "ons and offs," these irregularities in the curves can be got rid of, and at the same time, the magnetic susceptibility of the steel wires for low magnetising forces can be greatly increased. Further details regarding these interesting points are difficult to describe in a few words, but can readily be understood on reference to the paper itself.

The results of the experiments performed upon the magnetisation of somewhat thick bars (from ·9 to ·95 square centim. in section) of malleable iron, hard-tempered steel, and cast-iron, are also recorded fully in this paper. The intensity of magnetisation of each bar for various magnetising forces under different circumstances, is shown by means of curves, of which the "direct-curves" represent the results obtained by beginning with a low magnetising force, which was gradually increased to such a high degree of strength as to magnetise the bar to saturation; while the "return-curves" represent the results arrived at by coming down from a large magnetising force to smaller and smaller forces, passing through the zero, and gradually going up to a large magnetising force on the negative side of the zero. The direct-curves prove that the intensity of magnetisation of the steel bar is slightly greater, at least for high magnetising forces, than that of the cast-iron bar, but is vastly smaller than that of the malleable iron bar for all magnetising forces. The maximum intensity of magnetisation of the soft iron, steel, and cast-iron bar, is found to be approximately 1,330, 860 and 770 respectively; while the smallest magnetising force giving that magnetisation is roughly 190, 450, and 400 respectively. The difference in the intensity of magnetisation of these bars is, no doubt, due to the fact that the soft iron bar is far superior in respect to magnetisability to both the hard-tempered steel bar and the cast-iron bar; although the difference that exists between the soft iron bar and the wires in the intensity of magnetisation for all magnetising forces is probably due mainly to the effects of the dimensions of the bar, as has been mathematically demonstrated by Green. But the chief point of interest lies in the return-curves; they show that in the case of each bar the magnetisation does not reverse until the magnetising force exceeds a certain negative value, and that this value is considerable even in the case of the soft iron bar, considerably greater in the case of the cast-iron bar, and still greater —enormously greater—in the case of the steel bar.

An illustration of the beauty of this magnetometric method by means of curves showing the change in the distribution of magnetism in a wire corresponding to the change in the magnetising force to which it is subjected, draws the paper to a close. The curves decidedly show that the magnetisation of the wire for a low magnetising force is far from being solenoidal, being stronger towards the centre, but that as the magnetising force is made higher and higher the distribution of magnetism in the wire tends more and more to uniformity, until it attains nearly, if indeed not quite, a solenoidal state when the magnetising force is so high as to give the wire the maximum magnetisation; thus confirming beyond all doubt what has been pointed out theoretically by Thomson ("Electrostatics and Magnetism," § 667) and indicated experimentally by Rowland.

III. "On Abel's Theorem and Abelian Functions." By A. R. Forsyth, B.A., Fellow of Trinity College, Cambridge, Professor of Mathematics in University College, Liverpool. Communicated by Professor Cayley, F.R.S. Received October 28, 1882.

(Abstract.)

The present paper is divided into two sections. The object of Section I is to obtain an expression for an integral more general than, but intimately connected with, that occurring in Abel's theorem. The latter, as enunciated by Mr. Rowe in his memoir in the Phil. Trans., 1881, is as follows:—If

$$\chi(x, y)=0$$

be a rational algebraical equation between x and y, then an expression can always be found for

$$\Sigma\int\frac{\mathrm{U}dx}{f(x)\dfrac{d\chi}{dy}}$$

where $f(x)$ is a function of x only, U is a rational algebraical integral function of x and y, and the upper limits of the series of integrals are the roots of the eliminant with regard to y of $\chi(x, y)=0$ and a function $\theta(x, y)$.

In the case here considered two equations of the degrees m and n respectively between three variables

$$\mathrm{F}_m(x, y, z)=0$$
$$\mathrm{F}_n(x, y, z)=0$$

are given (these alone being treated, as subsequent generalization to the case of r equations between $r-1$ dependent variables and one independent is obvious); and an expression is obtained for

$$\Sigma\int\frac{\mathrm{U}dx}{f(x)\mathrm{J}\left(\dfrac{\mathrm{F}_m,\ \mathrm{F}_n}{y,\ z}\right)},$$

the upper limits of the integrals being given by the roots of the equation arrived at by the elimination of x and y between F_m, F_n and an arbitrary equation

$$\mathrm{F}_p(x, y, z)=0,$$

or, what is the same thing, by the co-ordinates x of the points of intersection of the three surfaces represented by F_m, F_n, F_p.

Some preliminary considerations (in connexion with §§ 92 sqq. of Salmon's Higher Algebra) are adduced in reference to the eliminants of the three equations in each of the variables; thus if X be the equation in x obtained by eliminating z and y, it is expressed in the form

$$X=B_mF_m+B_nF_n+B_pF_p,$$

which afterwards proves useful. Then the ordinary case (above referred to) of Abel's theorem is treated on the lines laid down in Clebsch and Gordan's treatise on the Abelian functions; and under the guidance of this the more general form is investigated with the result

$$\Sigma\int\frac{U\,dx}{f(x)J\left(\frac{F_m,\ F_n}{y,\ z}\right)}=\Theta\left[\frac{1}{f(x)}\right].\ \Sigma\left\{\frac{U}{J}\log F_p\right\}+C,$$

Θ being the symbol introduced by Boole.

The remainder of the section is occupied with the discussion of two examples of this theorem. In Example I, by the assumption of suitable forms for F_m, F_n, F_p, it is proved that

$$E(u_1)+E(u_2)+E(u_3)-E(u_1+u_2+u_3)=-\frac{8k^2ABC}{(A^2+B^2-k^2C^2)^2+4k^2A^2C^2}$$

where E is the second elliptic integral and A, B, C are given by

$$As_1+Bc_1+Cd_1=1,$$
$$As_2+Bc_2+Cd_2=1,$$
$$As_3+Bc_3+Cd_3=1,$$

and s, c, d stand for *sn u*, *cn u*, *dn u* respectively. The corresponding expression for the third elliptic integral is stated.

In Example II an expression is obtained for

$$E(u_1+u_2+\ \ldots\ldots\ +u_7).$$

In Section II the addition theorem for the functions presented in Weierstrass's memoir in Crelle, t. lii (1856), p. 285, is investigated. It may be pointed out that the fundamental equations in the theory occur as natural examples of the more general form of Abel's theorem proved in Section I; but the equations so obtained are identical with those used by Weierstrass, and this case, therefore, does not belong distinctively to the form of Abel's theorem connected with the curve of double curvature. On this account the simpler form is used on the two occasions (in §§ 14, 19) when required.

The theory is worked out at considerable length, and the necessary formulæ are obtained in a manner somewhat different from that of Weierstrass.

The fundamental equations being

$$y^2-\mathrm{P}(x)=y^2-(x-a_1)(x-a_2)\ \ldots\ldots\ (x-a_\rho)\ \ =0,$$
$$z^2-\mathrm{Q}(x)=z^2-(x-a_{\rho+1})(x-a_{\rho+2})\ \ldots\ (x-a_{2\rho+1})=0,$$
$$0=\mathrm{M}y+\mathrm{N}z$$

where

$$\mathrm{M}=x^\rho+\mathrm{M}_1x^{\rho-1}+\ \ldots\ldots\ +\mathrm{M}_{\rho-1}x+\mathrm{M}_\rho,$$
$$\mathrm{N}=\qquad \mathrm{N}_1x^{\rho-1}+\ \ldots\ldots\ +\mathrm{N}_{\rho-1}x+\mathrm{N}_\rho,$$

the equation giving the roots x is

$$\mathrm{M}^2y^2-\mathrm{N}^2z^2=0.$$

The 3ρ roots are denoted by $x_1, x_2, \ldots, x_\rho$; $\xi_1, \xi_2, \ldots, \xi_\rho$; $p_1, p_2, \ldots, p_\rho$; and there are obviously ρ relations between them. Writing

$$\mathrm{R}(x)=\mathrm{P}(x)\mathrm{Q}(x),$$

and

$$u_\mu=\tfrac{1}{2}\sum_{\lambda=1}^{\lambda=\rho}\int_{a_\lambda}^{x_\lambda}\frac{\mathrm{P}(x)dx}{(x-a_\mu)\sqrt{\mathrm{R}(x)}}\quad (\mu=1, 2, \ldots, \rho),$$

and v, w corresponding functions of ξ, p, it is shown that

$$u_\mu+v_\mu+w_\mu=0.$$

Writing, with Weierstrass,

$$\phi(x)=(x-x_1)(x-x_2)\ \ldots\ldots\ (x-x_\rho),$$
$$-\mathrm{Q}(a_r)=l_r\qquad (r=1, 2, \ldots\ldots, \rho),$$
$$\mathrm{P}(a_{\rho+s})=l_{\rho+s}\quad (s=1, 2, \ldots\ldots, \rho+1),$$

then $2\rho+1$ of the functions of the theory are given by

$$l_r al_r^2=\phi(a_r)$$

for values $1, 2, \ldots\ldots, 2\rho+1$ of r. Then if

$$\mathrm{U}=\tfrac{1}{2}\sum_{\lambda=1}^{\lambda=\rho}\int_{a\lambda}^{x_\lambda}\frac{\mathrm{P}(x)dx}{\sqrt{\mathrm{R}(x)}},$$

it is proved that

$$\frac{l_\mu al_\mu^2}{\mathrm{P}'(a_\mu)}=-\frac{d\mathrm{U}}{du_\mu}.$$

If V, W are respectively the same functions of the ξ's and p's as U is of the x's, then the theorem

$$\mathrm{U}+\mathrm{V}+\mathrm{W}=\sum_{m=1}^{m=\rho}\frac{l_m}{\mathrm{P}'(a_m)}al_m(u)al_m(v)al_m(u+v)$$

is obtained in § 21, a verification being furnished by expansion in terms of the u's and v's. From this equation is deduced the addition-theorem for the functions.

In §§ 25 and 26 is given the discussion of a particular case of the above, viz., that in which the functions are of the order 2, the fifteen functions being the quotients of all but one of the double theta-functions by that one. The addition-theorem in these functions has already formed the subject of a paper by Cayley in Crelle, t. lxxxviii (1878), p. 74.

IV. "Note on the Recent and Coming Total Solar Eclipses." By J. NORMAN LOCKYER, F.R.S. Received November 17, 1882.

The following note has been drawn up in anticipation of the detailed accounts of the work done by me in Egypt on the eclipsed sun of 1882, May 17, which I am preparing to lay before the Royal Society, because as the next total eclipse occurs next May, there is no time to be lost if any attempt is to be made to secure observations, and I am of opinion that such observations are most important.

I have prefaced the statement of the work done by a reference to the considerations which led me to undertake it, and I have added a scheme of observations which, in the present state of our knowledge is, I think, most likely to produce results of value.

1. In order to understand the recent change of front in solar research which has followed the introduction of the view of the possible dissociation of elementary bodies at solar temperatures, and suggested the later laboratory, and especially the later eclipse observations with which we are now chiefly concerned, we must first consider what facts we may expect on the two hypotheses. In this way we can see which hypothesis fits the facts best, and whether there are any inquiries possible during eclipses of a nature to throw light on the question.

2. On the old hypothesis the construction of the solar atmosphere was imaged as follows:—

(1.) We have terrestrial elements in the sun's atmosphere.

(2.) They thin out in the order of vapour density, all being represented in the lower strata, since the solar atmosphere at the lower levels is incompetent to dissociate them.

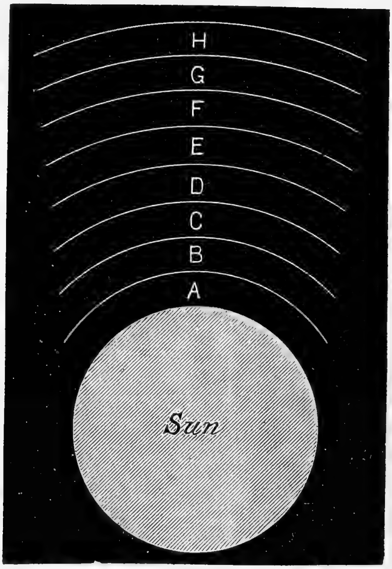
(3.) In the lower strata we have especially those of higher atomic weight, all together forming a so-called "reversing layer" by which chiefly the Fraunhofer spectrum is produced.

3. The new hypothesis necessitates a radical change in the above views. According to it the three main statements made in paragraph 2 require to be changed as follows:—

(1.) If the terrestrial elements exist at all in the sun's atmosphere they are in process of ultimate formation in the cooler parts of it.

(2.) The sun's atmosphere is not composed of strata which thin out, all substances being represented at the bottom; but of true strata like the skins of an onion, each different in composition from the one either above or below. Thus, taking the sun in a state of quiescence and dealing only with a *section*, we shall have (as shown in fig. 1) C say containing neither D nor B, and B containing neither A nor C.

Fig. 1.

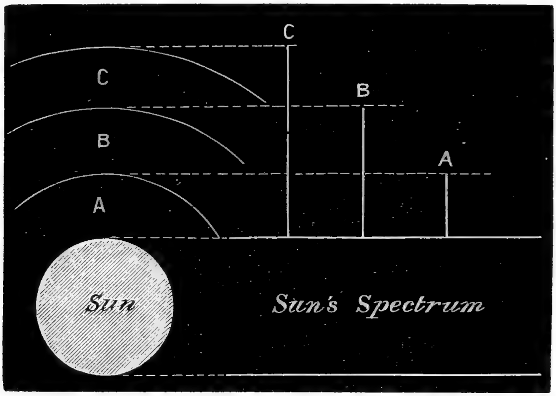


(3.) In the lower strata we have not elementary substances of high atomic weight, *but those constituents of all the elementary bodies which can resist the greater heat of these regions.*

4. The conditions under which we observe the phenomena of the sun's atmosphere have not, as a rule, been sufficiently borne in mind, and it is quite possible that the notion of the strata thinning out has, to a certain extent, been based more upon the actual phenomena than upon reasoning upon the phenomena.

5. Take three concentric envelopes of the sun's atmosphere, A, B, C (fig. 2), so that C extends to the base of A, and B also to the base of A, that is, in both cases to the photosphere. Then, whether we deal with the sphere or with a section of it, the lengths of the lines in the spectrum of the strata C, B, A will give the heights to which the strata extend from the sun, and show where B and A respectively thin out. As the material is by hypothesis continuous down to the

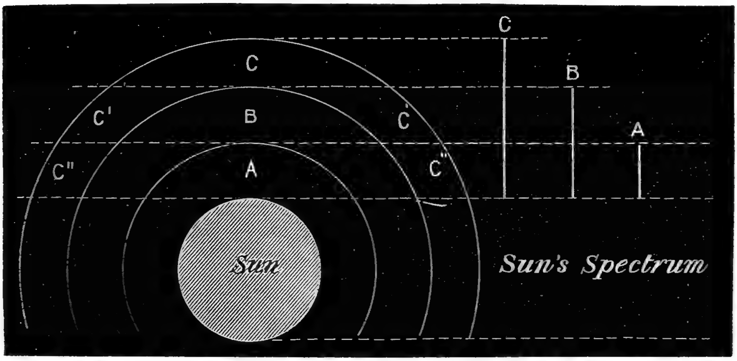
FIG. 2.



sun, the lines will be continuous down to the spectrum of the sun seen below as shown.

6. Now take three concentric envelopes, A, B, C (fig. 3), so that only A rests on the photosphere, B rests on A, and C on B. The phenomena will *in the main* be the same as in the former case, *i.e.*, the line C will still appear to rest on the spectrum of the photosphere, for it will be fed, so to speak, from C′ and C″, though absent along the line CBA at B and A. So also with B.

FIG. 3.



7. Thus much having been premised with regard to the observations as conditioned by the fact that we are observing a sphere, we can now proceed to note *how the two hypotheses deal with the facts.*

Old Hypothesis.	*New Hypothesis.*
1. The spectrum of each element as seen in our laboratories should be exactly represented in the solar spectrum.	The spectra should *not* resemble each other.

Fact.—There is a very wide difference between the spectra.

2. Motion in the iron vapour, *e.g.*, in a spot or a prominence, should be indicated by the contortion of all the iron lines equally.	Motion should be unequally indicated, because the lines are due to divers constituents which exist in different strata according as they can resist the higher temperatures of the interior regions.

Fact.—The indications show both rest and motion.

3. The spectrum of iron in a prominence should be the same as the spectrum of iron in a sun-spot.	The spectrum of iron in a prominence should be vastly different from the spectrum of iron in a sun-spot, because a spot is cooler than a prominence.

Fact.—The spectra are as dissimilar as those of any two elements.

4. The spectra of spots and prominences should not vary with the sun-spot period.	The spectra should vary, because the sun is hotter at maximum.

Fact.—They do vary.

5. The spectrum of the base of the solar atmosphere should most resemble the ordinary Fraunhofer spectrum.	The spectrum of the base should least resemble the Fraunhofer spectrum, because at the base we only get those molecules which can resist the highest temperatures.

Fact.—As a rule the lines seen at the base are either faint Fraunhofer lines, or are entirely absent from the ordinary spectrum of the sun.

6. *Quâ* the same element the lines widest in spots should always be the same.	*Quâ* the same element the lines widest in spots should vary enormously, because the absorbing material is likely to originate in and to be carried to different depths.

Fact.—There is immense variation.

7. The spectra of prominences should consist of lines familiar to us in our laboratories, because solar and terrestrial elements are the same.	The spectra of prominences should be in most cases unfamiliar, because prominences represent outpourings from a body hot enough to prevent the coming together of the atoms of which our chemical elements are composed.

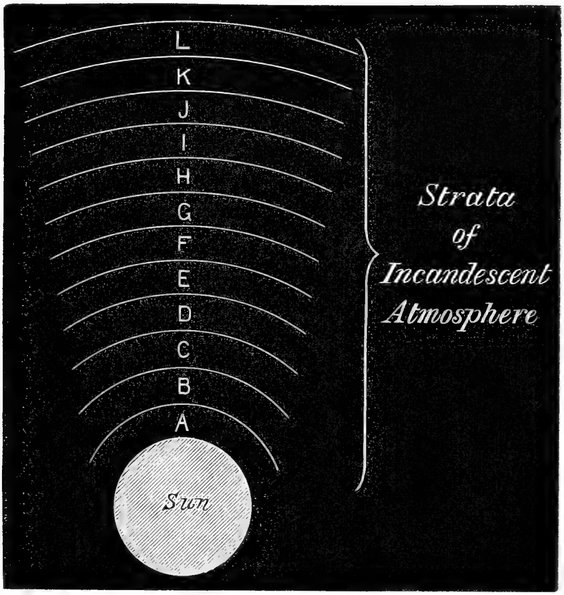
FACT.—When we leave H, Mg, Ca, and Na, most of the lines are either of unknown origin or are feeble lines in the spectra of known elements.

8. From the above sketch, hasty though it be, it is I think easy to gather that the new view includes the facts much better than the old one, and in truth demands phenomena, and simply and sufficiently explains them, which were stumbling blocks and paradoxes on the old one.

This being so, then, it is permissible to consider it further.

9. Let us first suppose, to take the simplest case, that the sun when cold will be a solid mass of one pure element, *i.e.*, that the evolution brought about by reduction of temperatures shall be along one line only. Let us take iron as the final product. Then the sun's atmosphere on the new theory *quâ* this one element may be represented as follows:—

FIG. 4.



Assume strata A—L. Then—

(1.) The Fraunhofer spectrum will integrate for us the absorption of all strata from A to L.

(2.) The darkest lines of the Fraunhofer spectrum will be those absorbed nearest the outside of the atmosphere.

(3.) We shall rarely, if ever, see the darkest lines affected in spots and prominences.

(4.) The germs of iron are distributed among the various strata according to their heat-resisting properties, the most complex at L, the least complex at A.

(5.) Whatever process of evolution be imagined, as the temperature runs down from A to L, whether A, 2A, 4A; or A+B, 2[2(A+B)], or X+Y+Z, the formed material or final product is the work of the successive associations rendered possible by the gradually lowering temperature of the successive strata, and can therefore only exist at L.

10. Now at this point a very important consideration comes in. It was stated (in 6) while discussing the conditions of observation, that whether we were dealing with strata of substances extending down to the sun or limited to certain heights, the spectral lines would always appear to rest on the solar spectrum, and that the phenomena would *in the main* be the same.

11. This, however, is true in the main only, there must be a difference, and this supplies us with a test between the rival hypotheses of the greatest stringency. The stratum B, being further removed from the photosphere than the stratum A, will be cooler, its lines therefore will be dimmer, and the lines of C will be dimmer than the lines of B, and so on. So if we could really observe the strata, *the longer a line is, i.e.*, the greater the height at which the stratum which gives rise to it lies, *the dimmer the line will be.*

12. Now our best chance of making such an observation as this is during a total eclipse. We do not see the lines ordinarily in consequence of the illumination of our air. As during an eclipse before totality the intensity of this illumination is rapidly diminishing, the lines first visible should be short and bright, and should remain short while the new lines which become visible as the darkness increases should be of gradually increasing length, so that the spectrum should become richer in the way indicated in fig. 5.

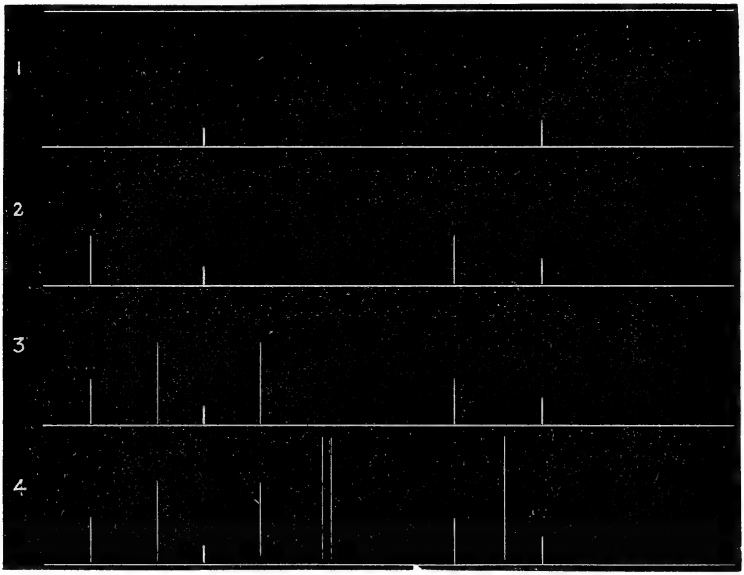
13. Further, the lines in 1 should be lines seen in prominences, and not in spots, and relatively brighter in the spark than in the arc, while the longer lines added in 2 and 3 should be lines affected in spots, and *not* in prominences.

14. All these phenomena were predicted for the Egyptian eclipse a year before its occurrence, and were verified to the letter for the lines of iron over a purposely limited region.

15. The actual observations of the iron lines made at Sohag are shown in the accompanying map, and these actual observations are contrasted with the lines thickened in spots, the lines observed in the prominences by Tacchini, those intensified on passing from the arc to the spark. The Fraunhofer lines are also given according to Ångström and Vogel, and the iron spectrum of the arc and spark according to Ångström and Thalén. The observations during the eclipse were made 7 minutes, 3 minutes, and 2 minutes before

totality as the air was gradually darkened, by which darkening successive veils, as it were, were lifted so that the more delicate phenomena could be successively seen.

Fig. 5.

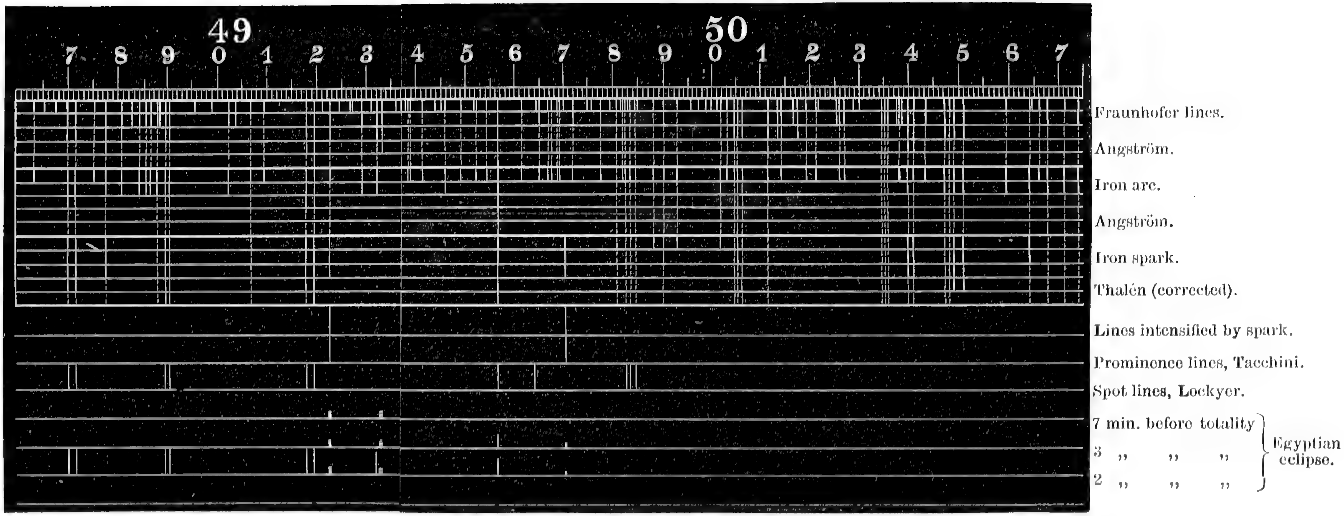


16. We begin with one short and brilliant line constantly seen in prominences, never seen in spots. Next, another line appears, also short and brilliant, constantly seen in prominences, and now, for the first time, a longer and thinner line appears, occasionally noted as widened in spots, while last of all we get very long, very delicate relatively, two lines constantly seen widened in spots, and another line not seen in the spark and never yet recorded as widened in the spots.

17. The procession from the hot to the colder is apparent, and the simplicity of the spectrum as opposed to the Fraunhofer spectrum even yet, is eloquent of the gradual approximation which would be still possible if the darkness could be greater and our attack more complete.

18. It will be noted over what an excessively small range the observations extend. We want similar observations over a wider range during future eclipses, and to do this work properly many observers armed with similar instruments must divide the whole or part of the solar spectrum amongst them, preferably that part between F and D, which has been most closely watched in prominences and spots by Tacchini and myself.

FIG. 6.



The Ba line at λ 4933·4 is a line seen thirty times by Young at the maximum spot period, and not recorded by Tacchini at the minimum. The lower longer iron line not seen till five minutes afterwards is a Te line λ 4932·5

19. I next pass to another point on which an observation was made in Egypt.

20. In fig. 4 we considered the sun's atmosphere, taking the simplest case, that of one element; when the sun cools it will be a very complex mass chemically. If the laws of evolution hold we need not expect that this will largely increase the complexity of the hottest layers A and B, but higher up, say at H—L, the complexity of chemical forms produced by evolution along the fittest lines will be very considerable.

21. These strata H—L may be taken to represent the corona. Its spectrum, therefore, should not be a continuous one, but should consist of an integration of all the radiations and absorptions of these excessively complex layers.

22. The spectrum of the corona as I saw it in Egypt exactly answered to this description. Instead of the gradual smooth toning seen, say in the spectrum of the limelight, there were maxima and minima producing an appearance of ribbed structure, the lines of hydrogen and 1474 being, of course, over all. This observation, however, requires confirmation, for the look I had at the corona spectrum was instantaneous only.

23. This observation should certainly be repeated during future eclipses with the proper instrumental conditions, *i.e.*, small, intensely bright image on narrow slit and spectroscope of small dispersion. I believe that, under these conditions, photographs could readily be obtained with the new plates.

24. Now an eclipse occurs next May at a critical time of the sun's activity, for, so far as we can see, we shall be nearly at sun-spot maximum, and I hold that it will be a disgrace to our nineteenth century science if efficient steps are not taken by those who are regarded as the leaders of science in this and other civilised countries to secure adequate observations.

25. So far I have only referred to those special observations undertaken this year to discriminate between two rival hypotheses, but both hypotheses may be wrong in many points, so that we must not limit ourselves to such observations, but collect facts over the whole field, as has always been the custom in eclipse expeditions.

26. In my opinion the following scheme shows the observations which, in the present state of our knowledge, it is *most desirable* to secure. The scheme, I am aware, is by no means exhaustive. I give the observations in the order of importance I attach to them, having regard to the present position of solar theory and the conditions of eclipse observations.

(1.) 6-inch equatorial of long focus, perfect clockwork, spectroscope with dispersion of at least five prisms of 60°.

Clamp point of disappearance of sun at base of normal slit, and record phenomena observed from ten minutes before totality to actual totality.

a. Order in which lines appear.

b. Brightness and length when first visible.

The spectrum from λ 4800 to λ 5900 should be distributed among at least five observers.

Repeat observations after totality on point of reappearance.

(2.) 6-inch photographic lens of four feet focus, perfect clock, same dispersion as above.

Clamp point of disappearance of sun on centre of tangential slit and record phenomena observed from ten minutes before totality to actual totality.

a. Order in which lines appear.

b. Brightness and length when first visible.

Repeat observations after totality on point of reappearance. Same part of spectrum, same distribution as in (1).

(3.) 6-inch photographic lens as in (2).

Photographic phenomena before and after totality on slowly ascending or descending or rotating plate, taking care to expose only narrow strip of plate.

(4.) Ditto. Spectroscope of small dispersion, long slit.

Photograph spectrum of corona during totality on both sides of dark moon.

(5.) Prismatic camera. 6-inch photo. lens as in (2), but with grating.

Use first order spectrum on one side and second order on the other.

Commence two minutes before totality. Continue till two minutes after totality on gradually ascending or descending or rotating plate.

(6.) 6-inch photo. lens as in (2), mounted on alt-azimuth. Fine slit. One prism of 60°. To observe spectrum of corona.

(7.) Photographs of corona of short, medium, and very long exposure to determine form and true solar limit of apparent corona due to the illumination of our air, using for the latter purpose the photographic intensity of the image of the moon.

I am aware that because Solar Physics is a new subject, and one so entirely in the domain of pure science, the above scheme may appear ridiculous to many, for if carried out in its completeness its cost would perhaps amount to the sixtieth part of the sum expended on the Transit of Venus in 1874. I have, however, felt myself bound to put it forward as an ideal scheme and one which, if several civilised Governments do each a little, concerted action may help us in part to realise. I am informed that the French and Italian Governments are already making preparations for observations, and my desire is that we may be represented on an occasion which, having regard to the duty which is incumbent upon us to secure observations for the use of those who come after us, is one of high importance.

November 30, 1882.

ANNIVERSARY MEETING.

THE PRESIDENT in the Chair.

The Report of the Auditors of the Treasurer's Accounts on the part of the Council was presented, by which it appears that the total receipts during the past year, including balances of £2,259 4*s.* 7*d.* carried from the preceding year, and the sum received from the Acton Estate, amount to £40,350 12*s.* 2*d.*; and that the total expenditure in the same period, including purchase of stock, and deposits, amounts to £38,409 17*s.* 11*d.*, leaving a balance at the Bankers' of £1,929, and £11 14*s.* 3*d.* in the hands of the Treasurer.

The thanks of the Society were voted to the Treasurer and Auditors.

The Secretary read the following Lists:—

Fellows deceased since the last Anniversary.

On the Home List.

Adams, Andrew Leith, LL.D.
Alderson, Sir James, Knt., M.D.
Ansell, Charles, F.S.A.
Balfour, Francis Maitland, M.A.
Binney, Edward William, F.G.S.
Budd, George, M.D.
Burton, Decimus, F.S.A.
Darwin, Charles, M.A.
Dickie, George, M.D.
Gulliver, George, F.R.C.S.
Harrowby, The Right Hon. Dudley Ryder, Earl of.
Jevons, William Stanley, LL.D.
Llewelyn, John Dillwyn, F.L.S.
Newmarch, William.
Parish, Sir Woodbine, K.C.H.
Robinson, Rev. Thomas Romney, D.D.
Russell, John Scott.
Smith, Col. John T.
Thomson, Sir Charles Wyville, LL.D.
Thwaites, George Henry Kendrick, C.M.G.

On the Foreign List.

Decaisne, Joseph.
Liouville, Joseph.
Schwann, Theodor.
Wöhler, Friedrich.

Defaulter.

Armstrong, Henry Edward, Ph.D.

Withdrawn.

Farr, William, C.B., M.D., D.C.L.

Fellows elected since the last Anniversary.

H.R.H. The Duke of Edinburgh, K.G.

Ball, Valentine, M.A. (Dubl.).
Brady, George Stewardson, M.D., F.L.S.
Bramwell, Right Hon. George William Wilshere, Lord.
Buchanan, George, M.D., F.R.C.P.
Clarke, Charles Baron, M.A., F.L.S., F.G.S.
Darwin, Francis, M.A., M.B., F.L.S., F.Z.S., F.R.M.C.S.
Dittmar, William, F.C.S., F.R.S.E.
Fawcett, Right Hon. Henry, M.A.
Gaskell, Walter Holbrook, M.A., M.D.
Glazebrook, Richard Tetley, M.A.
Godman, Frederic Ducane, F.L.S., F.G.S., M.E.S.
Harcourt, Right Hon. Sir William George Granville Venables Vernon, Knt., M.A.
Hutchinson, Jonathan, F.R.C.S.
Liversidge, Archibald, F.G.S., F.C.S., F.L.S.
Malet, John, C., M.A.
Mundella, Right Hon. Anthony John, F.R.G.S.
Niven, William Davidson, M.A.
Palgrave, Robert Henry Inglis, F.S.S.
Weldon, Walter, F.C.S., F.R.S.E.

The President then addressed the Society as follows:—

Our anniversary is in one sense the opening of a new year, in another it is the close of an old one. With one hand we welcome the coming, with the other we bid farewell to the departing guest. In the later parts of my present address I shall have to speak, as on former occasions, of our prospects and hopes for the future. At our more festive gathering in the evening we shall recount some of the victories which have been won over difficulties in the extension of knowledge, and shall rejoice at the gathering of old comrades and friends after our usual period of dispersion. But at the moment of taking my place in the chair to which you have now for the fourth time elected me, I must confess that the sadder side of the picture is the most prominent. We seem almost for the moment to enter the Valley of the Shadow of Death, or, like Dante, to descend to the place of Departed Spirits, and to commune with them once more after they have vanished from the upper world. Each year during my own term of office the numbers lost to us have been greater than the numbers gained; but this year, although the list of deaths is long and comprises not a few distinguished Fellows, they all seem overshadowed by two prominent figures. One of these died in the fulness of years, of honours, and of world-wide reputation; the other in the strength and buoyancy of youth, a buoyancy which appears to have even contributed to his end.

Of Darwin and his works it is not for me to speak. Others, with wider knowledge, after longer intercourse, and with greater authority, have said what was possible at the moment, and the full story of his life is now being written by faithful hands. But I consider it no common piece of fortune to have lived within easy distance of his house: to have been able by a short pilgrimage to enjoy his bright welcome, and his genial conversation, and to revive from time to time a mental picture of that my ideal of the philosophic life.

Of Balfour I knew far less, and his works are beyond my range of knowledge. But such was the fascination of his speech and his demeanour that to have seen him was to desire to know him better. To have been selected at his age as one of the Secretaries of the British Association, a post usually reserved for men of more advanced years and of longer experience, to have been appointed to a professorship founded almost on the basis of his own work, and thereby to have become the coadjutor of his own great master in the Physiological School at Cambridge, and all this without one word of cavil or of criticism, was a high testimony to his scientific eminence. But far wider afield, it will be remembered of him, not so much that he was brilliant in intellect, or keen of insight, or varied in his attainments, but that he always found himself among friends, whether in college or in the laboratory, in his own home over the northern border, or on the wild mountain side where he breathed his last.

The list of deceased Fellows comprises other eminent names, many of whom will receive mention in our obituary notices. The list, moreover, serves again to exemplify the variety of qualifications which have opened our doors to election. In Decimus Burton we find an architect of refined taste and cultivated mind; in Stanley Jevons and William Newmarch statisticians of weight, and the former already an authority on political and other philosophy; in Sir Woodbine Parish a geographer, and more than a geographer, a man who by service as well as by study in foreign lands had acquired an unusual amount of first hand and accurate information; in Scott Russell an engineer whose brilliant early strokes of work will be remembered when the difficulties which entangled his later efforts have been long forgotten; in Dr. Robinson a veteran and mentor in science, whose work and whose judgment were alike sound. Of Sir Wyville Thomson mention will be made elsewhere.

To this list of names there was well nigh added yet another, namely, my own. An accident, under circumstances which the issue of events and more mature reflection have shown that I was hardly justified in incurring, has for some time past interfered materially with my usual avocations in life, and thereby, as I fear, with my usefulness to the Society. But the ready and efficient assistance of the other officers has, I doubt not, gone far to supply the deficiency. For myself, I am consoled by the kind expression of sympathy from many, some even unknown, friends; and by the consideration, ever present to my mind, that, except through a combination of circumstances over which I had certainly no conscious control, the result to myself might have been far more serious.

The total number of Fellows lost to our ranks during the past year is twenty-two on the home list (one of whom has withdrawn on account of growing infirmities), and four on the foreign list; a result, on the whole, not very different from that of last year.

Of these two fell young, and by accident. Of the remainder, two died between the ages of 50 and 60, four between those of 60 and 70, six between those of 70 and 80; and the remaining five attained ages between four score and 90.

In Liouville we have again lost a veteran mathematician; in Wöhler, a chemist whose years, numbered from the beginning of the present century, reach to a period almost prehistoric in the records of his science.

I am happy to report that the sale of the Acton estate has been completed; and that of the proceeds, amounting to £32,250, £17,000 has been invested in preference or guaranteed railway stock; and the remainder will be expended in the purchase of ground rents, partly in the city of London, and partly in the western suburbs. The income from the latter source, already representing a very fair interest on the outlay, may be expected materially to increase at the expiration of the existing building leases. Some additional expense was incurred this year in painting a portion of the Society's apartments. A considerable portion still remains to be painted, either next year, or at some not very distant period.

While on the subject of property, I should mention that Her Majesty has sanctioned "the continuance of the occupation of the Royal Observatory at Kew by the Royal Society," upon certain conditions, which have been accepted. The building will be devoted, as heretofore, to the use of the Kew Committee, whose work, it must be remembered, is provided for in the main by the Gassiot Fund.

Last year the Society accepted a portrait of Sir J. D. Hooker, painted by Mr. John Collier, at the request and at the expense of a considerable number of Fellows. I trust that the Society will approve the action of myself and a few others, in this year offering for our collection a portrait, by the same artist, of Mr. Joule.

Mr. A. Le Gros has presented to the Society a bronze medallion head, executed by himself, of the late Mr. Darwin; and Mr. Budgett has again enriched our funds by a gift of £100.

The Library has received many valuable contributions both from our Fellows and from others. Among the latter I may mention the completion of "The Lepidoptera of Ceylon," from the Government of Ceylon; G. Retzius' "Gehörorgan der Wirbelthiere," from the author; a new edition of Abel's works, from the Norwegian Government; and facsimile lithographs of some of the late Professor

Clifford's mathematical fragments, and the catalogue in two handsome volumes from the Public Library of Victoria.

The printing of the general part of our library catalogue is in progress, and has reached the letter W; and although, owing to unforeseen difficulties the hope expressed last year, that it would have been now finished, has not been fulfilled, yet there seems little doubt that early next year it may be in the hands of the Fellows.

On the completion of this work the Library Committee contemplate resuming another decade, 1874–83, of the great Catalogue of Scientific papers; and the President and Council trust that the success which has attended the publication of the eight volumes already in existence will justify the Treasury in undertaking the printing of the second supplement when the MS. has been prepared.

In the staff of the Society I have happily no change to report. Of the existing members my own feelings would impel me to say much more; but, while they would probably wish me to be silent, I trust they will pardon me in this one remark: that while recent changes make me less apprehensive of any future alterations, they at the same time make me hope that any alteration may be long postponed.

Although the number of papers presented to the Society during the past year, apart from their contents, does not convey any very important information, yet in continuation of past practice I may perhaps carry on the ten years' table. It is as follows, showing a slight diminution in the past year:—

1873	92	papers	received.
1874	98	,,	,,
1875	88	,,	,,
1876	113	,,	,,
1877	97	,,	,,
1878	110	,,	,,
1879	118	,,	,,
1880	123	,,	,,
1881	127	,,	,,
1882	109	,,	,,

Among the papers of this year, I may notice the elaborate research by Dr. Debus on "The Chemical Theory of Gunpowder," forming the Bakerian lecture; the careful and long-continued investigations by Professors Liveing and Dewar on the spectra of water, and of carbon, and of mixed vapours.

Nor must I omit mention of Dr. C. W. Siemens' bold and original theory of the conservation of solar energy, which has already given rise to so much discussion. It will be sufficient for me here to say that upon the questions therein raised the last word has been by no means said; and that, whether the theory be ultimately established, or whether, like a phœnix, it shall hereafter give rise to some other outcome from its own ashes, it will ever be remembered as having set many active minds at work, and will always have a place in the history of Solar Physics.

In Mathematics, definite integrals, and elliptic and the higher transcendents continue to occupy much attention, and in particular our "Transactions" contain an excellent contribution to the theta-functions of two variables, by Mr. Forsyth, of Liverpool. To the theory of invariants, Professor Malet, of Cork, has given a happy extension in the direction of linear differential equations; but it is unnecessary to speak in detail of papers which either already are, or will shortly be, in the hands of the Fellows. I will only add that the "Philosophical Transactions" for 1882 will probably exceed in bulk, and not yield in interest to, those of any former year.

Looking outside the circle of our own publications, there has been one step gained during the past year, which, although in some sense a matter of detail, is really of great importance and interest. I allude to the paper by Lindemann, "Ueber die Zahl π" ("Mathematische Annalen," Band xx, p. 213). It had long since been shown that both the numbers π and π^2 are irrational; but hitherto no proof existed of the impossibility of effecting the quadrature of the circle by means of the straight line and circle, and ruler and compasses. Regarded from an algebraical point of view, every such construction must depend upon the solution of a quadratic equation, or rather of a series of quadratics whereof the first has for its coefficients rational numbers, and the succeeding members of the series only such irrational numbers as occur in the solution of their predecessors. This being so, the final equation can always be transformed, by transposition of terms and squaring, into an equation of an even degree with rational coefficients. And, consequently, if it can be proved that π cannot be a root of any algebraic equation whatever with rational coefficients, the impossibility of the quadrature of the circle will be thereby also proved. Starting from Hermite's researches ("Comptes Rendus," 1873), in which he established the transcendental nature of the number e, Lindemann has supplied the proof required with reference to the number π. It must be admitted that the proof is neither very simple nor very easy to follow; and it remains only to be hoped that it may some day assume such a form as may influence the minds which still exercise themselves upon the hopeless problem of squaring the circle.

A most important change in the relations between the Society and the Government in respect of State aid to science has been made this year. It will be in the recollection of the Fellows that an experiment was made for a period of five years, during which the sum of £4,000 was annually voted to the Science and Art Department, to be distributed at the recommendation of the Government Fund Committee of the Royal Society. That experimental period terminated, as then mentioned in my address, last year. The grant to the Science and Art Department has been discontinued, and in place of it an addition of £3,000 per annum has been made to the Government grant, making £4,000 in all. In concluding this arrangement the following stipulations were agreed to. The increased grant is to be administered by a Committee identical with the late Government Fund Committee; a portion may be devoted to personal grants, subject, however, to special recommendations to the Treasury; and, lastly, unexpended balances may be carried forward from year to year, as has hitherto been the case with the old Government grant only. To the stipulation that the increased fund should be administered by the more extended committee the Society felt that no reasonable objection could be offered, because upon it the President and Council are represented in full, and the *ex officio* members are in the majority of cases Fellows of the Society. The object of the second stipulation was, so far as the Society is concerned, to secure at the outset for the personal grants the consent and support of the Treasury, and thereby to preclude the chance of objection being subsequently taken to any of our proposals under this head. The President and Council, however, recognising the importance of great caution in respect of personal grants, have of their own motion appointed a special sub-committee (in addition to the three previously existing), to which all personal applications recommended by any of the other sub-committees are specially referred, and without whose recommendation none can come before the General Committee. To the third mentioned point, viz., the power of retaining unexpended balances, the President and Council attach great value, because that power may enable the Committee to devote more of its funds than heretofore to some of the larger undertakings in scientific enquiry, leaving more of the smaller grants to the special funds already in existence in the hands of the Royal and other societies. The meetings of this Committee will probably take place twice a year, in May and November. In the present year it will not be possible to hold the second meeting before December, but there will be advantages in holding it hereafter in November, as the entire annual grants will then be made by the same Committee, and under the sanction of the same President and Council. In concluding these few remarks on the new arrangements, I cannot refrain from expressing my sense of the obligation under which the Society and Science at large are laid by the sympathetic and intelligent attention bestowed upon the subject by the then Financial Secretary of the Treasury, the late Lord Frederick Cavendish.

Among other subjects referred to the Royal Society by Public Departments, I may mention a request from the Board of Trade for advice upon the question of improving the existing means at the

Standard Office for the purpose of comparisons. At the request of the President and Council, Sir George Airy, Colonel A. Ross Clarke, and Professor Stokes acted as a Committee, and drew up a very careful report, the value of which was fully recognised by the Board of Trade. The report suggested certain improvements in the present arrangements; but, having reference to the duties of the Standard Office as defined by Act of Parliament, it was not considered necessary to insist upon extreme scientific accuracy, such, *e.g.*, as that attained by Colonel Clarke himself in his "Comparison of Standards" made at the Ordnance Survey Office at Southampton in 1866.

The arrangements for the observation of the Transit of Venus have been steadily progressing. The parties have now all started for their stations, after their period of training under the superintendence of Mr. Stone at Oxford. An adequate supply of instruments has been secured at moderate cost, and all the accessory parts have been procured and applied by the indefatigable care and forethought of our directing Astronomer.

The English Expeditions for the observation of the approaching Transit of Venus are organized as follows:—

Accelerated Ingress.

Madagascar Observers.—Rev. S. J. Perry.
Rev. W. Sidgreaves.
Mr. Carlisle.

Cape Observatory Observers.—Mr. Gill and Staff.

Aberdeen Road Observers.—Mr. Finlay, First Assistant of the Cape Observatory.
Mr. Pett, Third Assistant of the Cape Observatory.

Montagu Road Observers.—Mr. A. Marth.
Mr. C. M. Stevens.

Retarded Ingress.

Bermuda Observers.—Mr. J. Plummer.
Lieut. Neate, R.N.
Capt. Washington, R.E.

Jamaica Observers.—Dr. Copeland.
Capt. Mackinlay, R.A.
Mr. Maxwell Hall.

Barbadoes Observers.—Mr. C. G. Talmage.
Lieut. Thomson, R.A.

Besides the observers at these stations, the Canadian Government has arranged to place three 6-inch and some smaller telescopes in the field. Lieut. Gordon of Toronto was sent by the Canadian Government to England to make himself master of the proposed arrangements, and to secure the necessary instrumental equipment.

ACCELERATED EGRESS.

The stations for Retarded Ingress are also available for Accelerated Egress.

RETARDED EGRESS.

Brisbane Observers.—Captain W. G. Morris, R.E.
Lieut. H. Darwin, R.E.
Mr. Peek.

New Zealand Observers.—Lieut.-Col. Tupman, R.M.A.
Lieut. Coke, R.N.

Besides these observers sent specially from England, the Observatories at Melbourne and Sydney are most favourably situated for observing the Egress. The Directors of these Observatories, Mr. Ellery and Mr. Russell, have promised their co-operation, and their Governments have placed funds at their disposal to cover any necessary expenses.

Unless unfavourable weather should prevent the transit being seen at some of the stations, we may expect some nine or ten pairs of corresponding observations, both at Ingress and Egress, from the British expeditions alone. These observations are certain to be largely supplemented by those made by the observers of other nations; and it is hoped, from the close agreement between the instructions issued to the different observers, that the whole may ultimately be available for combination in one general discussion.

The American astronomers, encouraged by the partial success which attended the plan they adopted in 1874, are relying chiefly upon the photographic method; they have sent expeditions to South America and the Cape of Good Hope.

Austria does not take any active part in observing the Transit.

France sends out eight well equipped expeditions, full particulars of which have been published in the "Comptes Rendus" for October 2.

From Holland no special expedition will be sent out, but Lieutenant Heyming, of the Dutch Navy, will observe the transit in the West Indies, probably at Curaçoa.

Italy will confine its operations to observatories in that country.

Russia, also, has decided to send out no expeditions of its own, but it has aided the efforts of other countries by lending a 6·5-inch reflector to the Danish Government, and has placed two excellent 4·3-inch heliometers in the hands of the French astronomers, MM. Tisserand and Perrotin. The considerations which led the Russian Government to this conclusion have been explained in the following paragraphs of a letter from Mr. Struve to myself:—

"Experience since 1874 has sufficiently proved that there is no prospect whatever, even with combined international efforts, of obtaining by the present transit a geometrical determination of the parallax of the sun, which would not soon be surpassed in accuracy by other recent methods (for example, that suggested by Mr. Gill), methods which are capable of being repeatedly employed, and that without any costly expeditions.

"Further, although it must be admitted that so rare an opportunity of studying the atmosphere of the planet ought not to be neglected, yet it seems certain that many and excellent data will be obtained through the agency of the United States, as well as by other countries having well provided observatories in the southern hemisphere, as well as by other seafaring nations." Under these circumstances Russia has not considered it incumbent on itself to organise any observing parties.

Spain has sent two parties of naval officers, well equipped with 6-inch equatoreals and other instruments, to the Havana and Porto Rico.

Last year I expressed a hope that the difference of longitude between Singapore and Port Darwin in Australia would be determined by Commander Green of the United States' Navy in concert with Mr. Todd. This operation, however, in consequence of some incorrect information furnished to Commander Green as to the intentions of our home authorities in the matter, was not carried out. After various proposals, extending over a period of not less than two years, I am happy to say that it now appears likely that the work will be performed. Through the liberality of the Secretary of State for War an extension of leave has been granted to Lieutenant Darwin, who accompanies Captain Morris to Brisbane to observe the transit of Venus, enabling him to undertake the work. He has received instructions to arrange with Mr. Todd all details of the operation. The publication of the results obtained by Oudemans and Pogson for the difference of longitude between Madras and Singapore has now left only one link wanting, namely, that between Batavia and Port Darwin, to connect Australian with English longitudes. Lieutenant Darwin is eminently qualified for the work; and it seems a happy coincidence that it should fall to his lot to connect astronomically the distant port named after his father with the furthest ascertained point in that direction. I should not omit to add that Mr. Todd has placed all the telegraphic appliances under his command at the disposal of this service, and it is to be hoped that the determination will prove as useful to the Australian colonies as it will be valuable for the purposes of the transit. The best thanks of the Committee have already been given, but I am glad here publicly to recognise the valuable assistance rendered to the Committee in these long negotiations by the Great Eastern Telegraph Company.

In the course of last year the Treasury made known to the Society that in consequence of Sir Wyville Thomson's ill health, their Lordships proposed that his chief assistant, Mr. Murray, should undertake the general editorship of the Reports of the "Challenger" Expedition; so that Sir Wyville might devote himself more exclusively to the personal narrative. At the request of their Lordships a small Committee, with whom Mr. Murray might consult from time to time, was appointed, consisting of the President and Officers, Sir Joseph Hooker and Professor Huxley; but before the Committee could meet the lamentable death of Sir Wyville Thomson occurred. They met, however, shortly afterwards, and having added Professor Moseley to their number, they received from Mr. Murray, who attended, a detailed statement of the existing condition of the whole arrangements connected with the Report. From this statement it appeared that, in addition to the original estimate of £20,000 given by Sir Wyville Thomson, the work actually in progress and entrusted to the several authors required a further sum of about £20,000, and that if the series should be completed, by describing on the same scale groups as yet unallotted, an additional expense of about £6,000 would be entailed. In forwarding this statement to the Treasury, the Committee stated that, in their opinion, Mr. Murray's estimates were drawn up with great care and judgment, and that in view of the remaining Reports being carried out on the same scale as those already published, they were reasonable and sound. As to the cause of the great discrepancy the Committee felt themselves unable to offer any explanation; the conduct of the whole business having been left in Sir Wyville's hands, without reference to the Society. They further were of opinion that Mr. Murray might safely be entrusted, under the control and supervision of the Committee, with the entire future management of the undertaking.

After some further correspondence it was suggested that Mr. Murray should furnish the Committee with a statement of the existing condition of the Reports and their management, which should form a starting point for the responsibility of the Committee; and that he should keep the Committee well informed from time to time of the progress of the undertaking. These suggestions were cordially accepted by their Lordships, and with the general statement which Mr. Murray submitted in October, the special duties and responsibilities of the Committee have begun.

Since last year, three more volumes of the Report have been published, making six in all. The new volumes form volumes iv and v of the Zoology, and volume ii of the Narrative. The latter volume comprises the magnetic results, the meteorological observations, the report on the pressure errors of the thermometers, and the petrology of St. Paul's rocks. Vol. i of this section, containing the narrative proper, is partly in type; and will, it is hoped, be issued during the summer of 1883. Other volumes also will appear from time to time.

In connexion with this subject, I may mention that the collection of specimens from the "Challenger" Expedition are being received at the British Museum, as the particular portions are released by the progress of the publication of the Report. Those derived from the "Alert" Expedition to the South Pacific Ocean, have been deposited in the Museum by the Admiralty, and are now being arranged and described. Dr. Günther hopes to be able to produce a printed descriptive catalogue of the collection before the expiration of the present year. And I desire here to acknowledge the service rendered to science by the Admiralty in commissioning Dr. Coppinger to accompany that expedition for scientific purposes.

I am indebted to Mr. Murray for the following interesting account of a cruise made last summer to complete some part of the "Challenger" work.

H.M.S. "Triton" was engaged, from the 4th of August to the 4th of September, in a re-examination of the physical and biological conditions of the Faroe Channel.

The chief objects of the cruise were to ascertain by actual soundings, the character of a ridge running from the north of Scotland to the Faroe fishing banks, and separating, at depths exceeding 300 fathoms, the cold Arctic water with a temperature about 32° from the so-called Gulf Stream water on the Atlantic side with a temperature of 47° F. This ridge was traced in considerable detail by means of cross soundings directly across the channel, and the top was found to be on an average about 260 fathoms beneath the surface. In the northern half of the ridge, however, a small saddle-back was found with a depth of a little over 300 fathoms, through which some of the Arctic water seemed to flow and to spread itself over the bottom on the Atlantic side of the ridge. The top of the ridge is entirely composed of gravel and stones, but mud and clay are found on either side at depths exceeding 300 fathoms. Many of the stones are rounded, and some of them have distinct glacial markings. They are fragments of sandstone, diorite, mica-schist, gneiss, amphibolite, chloritic rock, micaceous sandstone, limestone, and other minerals. The ocean currents here appear to be strong enough, at a depth of between 250 and 300 fathoms, to prevent any fine deposit, such as mud or clay, being formed on the top of the ridge. All the indications obtained of the nature of this ridge, seem to imply that it may be a huge (terminal ?) moraine.

It is worthy of notice that the "Wyville Thomson Ridge" is only a little to the east of the position marked out by Croll from the observations of Geikie, Peach, and others, as the probable limit of the perpendicular ice cliff formed in North Western Europe during the period of maximum glaciation.

The dredging captures show the same marked difference as had previously been pointed out in the fauna of the two areas; those in the cold area being of a distinctly Arctic character, and those in the warm area resembling the universally distributed deep-sea fauna of the great oceans. A fair proportion of new species were also found.

The last trip of the "Triton" took place from Oban, on the 11th September, to the deep water in the Atlantic westward of Ireland. The object of this trip was to get *directly* a determination of the pressure unit of the gauges employed in testing the "Challenger" thermometers. The original determinations were made *indirectly* by the help of Amagat's results as to compression of air. The observations taken are not yet reduced, but several successful trials were made at depths of 500, 800, and 1,400 fathoms.

The subject of the Circumpolar Observations mentioned in my address of last year, was since that time brought more formally before our Government by that of Russia. At the request of the Treasury, the President and Council, after consultation with the Meteorological Office, advised as follows:—

"The object of the undertaking is to throw light on the influence of the great inaccessible region surrounding the pole on the meteorology and magnetism of the earth. With this view it is proposed to take simultaneous observations at a chain of circumpolar stations for a full year at least.

"A chain of not less than eight stations will be occupied independently of any co-operation by this country. This chain, however, leaves a gap of 90° in longitude in the northern part of America, the centre of which would be advantageously occupied by a station in the Dominion of Canada. The value of the results will be greatly enhanced by the addition of this link to the chain. Independently of this, such a station would be of great value as being of a continental character, in contrast with the other stations, which are in close proximity to the coast. By choosing for the station one of the forts of the Hudson's Bay Company, no great outlay need be involved in its occupation."

The point first proposed was Fort Good Hope, near the mouth of the Mackenzie River; but it was found too late to erect the necessary huts and to transport the party and its provisions there during the present season. Fort Simpson, on the same river, was next suggested. Guided by considerations of facilities of access and sustentation, the Committee came to the conclusion that either Fort Rae or Fort Providence, on Great Slave Lake, is to be preferred to Fort Simpson, with which the former forts nearly agree in latitude; and accordingly the President and Council recommended one of these.

"In framing an estimate, it was thought well to assume that the expedition might last a year and eight months, so as to allow a sufficient margin for travelling to and from the station, and for possible detention in waiting for the Hudson's Bay Company's brigade. It is calculated that the cost might be safely estimated at £3,000, which would include salaries of one officer and three men; journey of the party from England and back, including reasonable baggage; rations, allowances, and all other expenses."

To this communication the following reply was received:—

"My Lords have to thank you, and the Committee whom the Council appointed to advise them in the matter, for the valuable information contained in Dr. Michael Foster's letter of the 16th ultimo. Acting upon that information and upon the advice of the Royal Society, Her Majesty's Government have decided that this is an object on which public money may properly be employed, and they are prepared to ask Parliament to provide a total sum not exceeding £2,500 for the purpose. My Lords understand that there is good reason to hope that the balance required to make up the total estimated cost of £3,000 will be forthcoming from other sources.

"I am to ask whether the Royal Society would be so good as to take charge of the Expedition under similar conditions to those under which the Transit of Venus Expedition is being conducted; accounts of the expenditure chargeable to the Parliamentary grant being rendered to this Department. The choice of stations, the appointment of observers, and the methods of procedure would be left entirely to the Society, subject to the condition that the total amount chargeable on public funds does not exceed £2,500. My Lords understand that it is expected that not more than £1,500 of this amount would come in course of payment during the present year, and they will present estimates to Parliament for £1,500 and £1,000 at the proper times."

The Canadian Government has since promised a contribution of 4,000 dollars towards the expenses of the expedition.

A committee, consisting of the President, Dr. Rae, Sir George Richards, Mr. R. H. Scott, and Professor Stokes, was accordingly appointed to superintend the expedition, which, comprising Captain H. P. Dawson, R.A., in command, Sergeants J. English and F. Cookesley as observers, and W. Wedenby, as artificer, left England on May 11, for Quebec, was heard of at Fort Carlton on 27th June, and was about to proceed the next day for Green Lake, on the way to Portage La Loche. It was still not quite certain whether it might not be necessary to push on to Fort Simpson, on account of insufficient accommodation, as well as lack of time and materials for building at Fort Rae.

Two parts of "Mittheilungen der Internationalen Polar Commission" have been published, containing full particulars and instructions relating to the whole circumpolar scheme.

The geological, mineralogical, and botanical collections, formerly in the Museum in Bloomsbury, have been properly arranged in the new building in Cromwell Road, and are on exhibition in their respective galleries. A commencement has been made in the transfer of the zoological collections. The osteological specimens, hitherto packed out of sight in an obscure vault in the basement of the old Museum, have been safely removed to the new building, and are now exhibited in a large and well lighted gallery. The collection of shells, which occupied the floor space of the long eastern gallery in Bloomsbury, is now suitably exhibited at South Kensington. Some of the corals have been removed, in order to clear the way for the removal of other specimens; and many of the stuffed quadrupeds and mammalian skins which had been stowed away in the old Museum basement are now in the new repository.

The removal of the general collection of mammalia, of the birds, of the entomological specimens, and those of British zoology, will not be undertaken until after the coming winter. The fittings for the galleries prepared for them are not fully completed. The detached building designed for the specimens preserved in spirit cannot be made ready for their reception before the opening of next spring. It is, however, expected that the whole of the zoological collections will have been transferred to the new Museum by the end of June, 1883.

The subject of Technical Education has continued to be prominently under the notice of the country during the past year. The appointment of a Royal Commission on Technical Instruction, to which I have previously referred, has done much towards awakening the interest of manufacturers, and exciting curiosity in regard to the efforts that are being made abroad to improve the education of artizans. The Commissioners issued in March last their first Report, which dealt exclusively with primary education and apprenticeship schools. The Commissioners expressed an opinion adverse to the establishment of apprenticeship schools in this country; and in this view they are supported by nearly all our large manufacturers, and by the action of the City and Guilds of London Institute for the Advancement of Technical Education. At the request of the Executive Committee, I myself gave evidence before the Commission, explaining generally the objects of the City Guilds and Institute, and describing the progress already made towards their attainment. As a member of the Executive Committee of this Institute, I have watched its progress with interest, and have observed with satisfaction that its scheme of Technical Instruction is being gradually matured. The general Examinations in Technology undertaken by this Institute, were held in May last at 147 centres in 37 subjects. Of the 1,972 candidates who presented themselves for examination, 235 passed in Honours, and 987 in the Ordinary Grade. In 1881, 895 candidates passed, showing an increase of 307. The Examinations were held this year for the first time under the revised Regulations, which appear to have worked very satisfactorily. Two points deserve notice with respect to these Examinations. In the first place, the Institute experiences very great difficulty in obtaining properly qualified teachers. The applicants are either practical men working in the factory, or at their trade with no scientific knowledge whatever, or men possessing a very elementary science knowledge, and little or no practical acquaintance with the details of the industry, the technology of which they profess to understand. In order to indicate the kind of qualifications required in an ordinary technical teacher, the Institute has inserted in its programme a paragraph to the effect that persons who are engaged in teaching science under the Science and Art Department, and who at the same time have acquired a practical knowledge of their subject in the factory or workshop, may be registered as teachers of the Institute. The second point calling for consideration is the fact referred to in the Report of the Directors,—that of the 1,222 candidates who, this year, passed the examinations, most of whom are workmen or foremen in various branches of industry, not more than 450 are qualified to receive the full Technological Certificate, by having previously passed the examinations of the Science and Art Department in certain science subjects. This fact clearly indicates that widely beneficial as has been the action of this Department of State, there is still a large field for its influence among the population who are engaged in manufacturing processes, and desire to receive Technical Instruction.

One of the most satisfactory results of the Examinations of the City and Guilds of London Institute is the impulse they have given to the establishment, in different parts of the country, of properly equipped technical schools. At Manchester, Preston, Dewsbury, Hawick, Sheffield, Leicester, and other places, efforts have been made during this year towards organising schools for the technical instruction of artizans and others in the application of science and art to specific industries. At Nottingham, a grant of £500 has been made by the Institute, to be followed by an annual contribution for a limited period of £300, towards the establishment of technical classes in connection with the University College; and at Manchester a subscription of £200 a year has been promised to assist the funds now being raised for the conversion of the Mechanics' Institution into a Technical School. The attention of the Council has been greatly occupied of late with the arrangements for the opening of the Finsbury College. Classes in Electrical Engineering and in Technical

Chemistry, have been carried on for nearly three years in temporary rooms belonging to the Cowper Street Schools. The attendance at these classes has been eminently satisfactory, much more so than could have been anticipated. During the past session 960 class tickets were sold at fees varying from 5*s.* to 12*s.* The staff of the College has recently been doubled by the appointment of a Professor of Mechanical Engineering, and a Head Master to the new Department of Applied Art, the establishment of which, as I stated last year, was then under the consideration of the Committee. In January next, it is anticipated that the new building in Tabernacle Row, which is already nearly completed, will be opened for the reception of students. The programme of instruction, prepared by the Director and the Professors of the College, has been for some time under the consideration of the Committee, and it is hoped that in the instruction given in this College will be found the realization of a very important part of the Institute's Scheme of Technical Education.

Grants to the Technical Science Classes at University College and King's College, London, to the Horological Institute, to the School of Art Wood Carving, and other institutions, have been continued during the past year.

The Technical Art School in Kennington Park Road, established and maintained by the Institute, has been satisfactorily attended; and a proposition is to be brought before the Committee for supplementing the teaching of this school by technical science classes, with the view of establishing in the south of London a Technical College for Artizans, similar to the one about to be opened in Finsbury.

The building of the Central Institution or Technical High School in Exhibition Road, the foundation stone of which was laid by H.R.H. the Prince of Wales, President of the Institute, in July, 1882, is rapidly advancing and promises to be completed within a year. It is not expected, however, that this school will be ready for the reception of the students before the commencement of the session 1884–5. Meanwhile, the Council and Committee are fully occupied with the development of other parts of their scheme.

In forwarding the Report of the Meteorological Council to the Treasury in December last, the President and Council took occasion to remind their Lordships that the arrangement for the organisation of the Meteorological Office generally, in May, 1877, would terminate with the then financial year. The Treasury, in reply, asked the advice of the Royal Society. After consultation with the Meteorological Council on various points connected with the subject, the President and Council reported fully to the Treasury, and concluded with the following general recommendation: "The President and Council beg leave to express a hope that the constitution of the Meteorological Council may remain unchanged, and that the same gentlemen who have hitherto performed its duties and administered its funds with such intelligence and judgment may be disposed to continue their labours." To this recommendation the Treasury cordially assented; deciding at the same time that no period should be fixed to the Meteorological Council for their tenure of office, but that it might be terminated by either party at any time on twelve months' notice.

The Meteorological Office has completed during the past year a series of charts of sea surface temperature, for the three great oceans of the globe, and for the representative months of February, May, August and November. The work, which is now in the course of publication, will consist of twelve large charts, for the Indian, Atlantic, and Pacific Oceans respectively; and of four on a reduced scale, showing, for the four months, the isothermal lines of sea surface temperature over the entire globe. In the preparation of these charts, all the observations existing in the Log Books of the Meteorological Office, and in the Remark Books of the ships of Her Majesty's Navy, have been employed, as well as the information which has been already rendered accessible in scientific memoirs, and in the narratives of the great scientific voyages. The isotherms agree substantially with those which have been already given for the months of February and August, in the wind and current charts published by the Hydrographic Department of the Admiralty; but as the present series is founded on a much larger number of observations than have ever before been available for a similar purpose, it may fairly be regarded as a valuable contribution to a not unimportant part of terrestrial physics. Between the limits of 50° north and 50° south latitude, the mean annual surface temperature, so far as it can be deduced from the data now available, appears to be 74°·9 F. for the Indian, 69°·5 F. for the Atlantic, and 68°·6 F. for the Pacific Ocean. The North Atlantic is 4°·6 F. warmer than the South Atlantic Ocean; the corresponding difference in the case of the Pacific Ocean is only 1°·8 F.

Among other contributions to Ocean Meteorology, which the past year has producod, I may mention (1) the Physical Charts of the Atlantic Ocean, published by the Deutsche Seewarte, at Hamburg; (2) the second volume of the narrative of the voyage of H.M.S. "Challenger," containing the magnetical and meteorological observations; and (3) a report by Captain Toynbee, F.R.A.S., on the Gales of the Ocean District adjacent to the Cape of Good Hope, which completes the discussion by the Meteorological Council of the meteorology of that tempestuous part of the sea.

The meteorology of our own country has been actively studied during the year. The Scottish Meteorological Society have given in their Journal a series of monthly pressure charts for the British Isles, together with a revised edition of the temperature charts already published by them in 1871. The charts now embody the results of observations extending over a period of twenty-four years; the revised edition, as well as the original publication, are due to the indefatigable activity of Mr. Alexander Buchan, F.R.S.E., the Secretary of the Scottish Meteorological Society. An atlas of convenient size, intended for the use of observers in the United Kingdom, and conveying similar information derived from data partly different, and quite independently discussed, has been already prepared by the Meteorological Office, and will immediately appear.

It is a fact now universally recognised that the greater part of the changes of weather which are experienced in the British Isles are occasioned by travelling areas of excessive or defective atmospheric pressure, which arrive at our shores from the Atlantic Ocean. The importance of a systematic study of the weather of the North Atlantic being thus indicated, the Meteorological Council have resolved to undertake the preparation of synoptic weather charts for the thirteen months beginning 1st August, 1882, and ending 31st August, 1883, and have issued a special appeal to the British shipping interest for active co-operation during that period. It is satisfactory to know that this appeal has not been fruitless, and that there is every prospect that the number of observations available for the discussion will exceed 200 per day.

This is, perhaps, the proper place to make mention of some results having an important bearing on meteorology, obtained by Professor Tyndall in the course of a larger research on the action of radiant heat on gases.

By methods which he has applied to gases and vapours generally, Tyndall has established anew the action of aqueous vapour upon radiant heat, and the sensibly perfect diathermancy of dry atmospheric air. The phenomena of solar and terrestrial radiation are profoundly modified by the presence of aqueous vapour in the earth's atmosphere, the temperature of our planet being thereby rendered very different from what it would otherwise be.

The celebrated experiments of Patrick Wilson, wherein were observed a rapidity of radiation and a refrigeration of the earth's surface previously unknown, are explained by the fact that when they were made the amount of aqueous vapour in the air was infinitesimal, the unhindered outflow of heat towards space being correspondingly great. The sagacious observation of Six and Wells, that the difference between the surface temperature and that of the air a few feet above the surface, on equally serene nights, is greatest in cold weather, is explained by the fact that, when the temperature is low, the agent which arrests the surface radiation is diminished in quantity. Wells, moreover, found that the heaviest dews were deposited on nights when the difference between air temperature and surface temperature was small; while the greatest difference between the two temperatures was observed on nights when the deposition of dew was scanty. The explanation offered by Tyndall is this: —copious dew indicates abundant vapour; and abundant vapour, by arresting the terrestrial rays, prevents the refrigeration observed in drier air. Strachey's able discussion of observations made at Madras, point distinctly to the action of aqueous vapour on the radiation both of the sun and of the earth; while the experiments of Leslie, Hennessey, Hill, and other distinguished men, which were long considered enigmatical, are readily explained by a reference to the varying quantities of vapour with which the atmosphere is charged, on days of equal optical transparency. The interesting observations of Desains and Branley, made simultaneously on the Rigi and at Lucerne, are well worthy of mention here. The difference of level between the two stations is 4,756 feet, and within this stratum 17·1 per cent. of the solar heat was proved to be absorbed. This absorption being due to aqueous vapour, is tantamount to the transmission of the sun's rays through a layer of water of a definite thickness. A sifting of the rays would be the consequence, and on *à priori* grounds we should infer that the percentage transmission through water at Lucerne must be greater than on the summit of the Rigi. This was the exact result established experimentally by Desains and Branley. Mr. H. Wild, Director of the Central Physical Observatory, St. Petersburg, basing his statement on experiments made by himself according to Tyndall's method, has expressed the opinion "that meteorologists may, without hesitation, accept this new fact in their endeavours to explain phenomena which hitherto have remained more or less enigmatical." The correctness of this statement is illustrated by the foregoing examples, to which, if necessary, many more might be added.

At the recommendation of the Committee on Solar Physics of the Science and Art Department, a grant of £350 was made from the Society's Donation Fund to Captain Abney and Mr. Lockyer in aid of their proposed observations of the total eclipse of the sun at Thebes in May last. Unfortunately the state of Captain Abney's health precluded his taking part in the expedition; but Dr. Schuster generously undertook the conduct of his observations, and, notwithstanding the short time remaining for preparation, he carried them out in the most satisfactory manner.

Three photographs of the corona itself were obtained during the eclipse. They show that the corona had the characteristic features observed during the time of the maxima of sun-spots. The long streamers in the plane of the ecliptic seen during sun-spot minima were absent, and the corona showed much disturbance. A bright comet appeared in all the photographs at a distance slightly less than a solar diameter.

A complete photograph of the spectrum of the prominences and the corona was for the first time obtained. The prominences give a spectrum in which the lines of calcium bear a conspicuous part by their intensity. The ultra-violet hydrogen lines, photographed in star spectra by Dr. Huggins, were seen, as well as a number of unknown lines.

The corona gives a very complicated spectrum. Close to the limb of the sun the spectrum was so nearly continuous and so strong as to hide any lines which might have been present. Further away the continuous spectrum fades off, the region of the solar group G appears occupied by an absorption band, and a large number of coronal lines hitherto unobserved appear in the ultra-violet.

In addition to these photographs one was obtained in a camera, in front of whose lens a prism was placed without a collimator. This photograph allows us to study the spectra of different prominences. As the picture was produced on one of Captain Abney's infra-red plates, all the tints of the prominences ranging from the ultra-red to the ultra-violet made their impressions, and some interesting differences in the spectra of different prominences can be noticed.

But, beside taking part in this expedition, Mr. Lockyer has continued with unwearied perseverance his observations on the spectra of solar prominences and spots, and has recently combined with these the results obtained by him during the late eclipse. During this eclipse he made naked eye observations, which he considers to be of a crucial character between the two rival hypotheses regarding the nature of the sun's atmosphere. The results of this investigation have in his opinion considerably strengthened the views which he first put forward in 1873 on the constitution of the solar atmosphere. A statement of these views will be found in a paper by him recently read before the Society.

In the present state of the questions there raised, it must I think be admitted that, after giving all due weight to the facts and reasonings adduced by Mr. Lockyer, additional and varied observations are greatly to be desired; and that no opportunity reasonably available, for adding to our knowledge of the subject, should be neglected. And, therefore, without committing myself or the Society to the support of any particular proposal or expedition, I think that it may be fairly claimed as a *primâ facie* duty on the part of the present generation to obtain as many faithful records of the various phenomena occurring during solar eclipses as possible.

From a discussion of the meridian observations of Mars made during the favourable opposition of 1877, at Washington, Leiden, Melbourne, Sydney, and the Cape, Professor Eastman has deduced the value $8''{\cdot}953$ for the solar parallax—a value which, though considerably larger than any of those found by other methods, agrees closely with that obtained by Mr. Downing, in 1879, from the meridian observations of Mars at Leiden and Melbourne, as well as with the values found from similar observations in 1862. In this investigation, Professor Eastman rejects the observations at Cambridge, United States, as they were made in a slightly different manner, and gives (in combination with Melbourne) a very large value for the solar parallax, viz., 9″·138.

The detailed account of the British Observations of the Transit of Venus, 1874, was published at the beginning of the year, and the observations of the transit made at colonial observatories have been recently printed in the Memoirs of the Royal Astronomical Society.

The Transit of Mercury last November was well observed in Australia and other places, and the results are of special interest in connexion with the coming Transit of Venus. The discordances in the times of internal contact recorded by different observers seem to show that such observations are subject to much uncertainty.

An important memoir on astronomical refraction has been lately published by M. Radau, who, after a discussion and comparison of previous theories, gives formulæ and tables for refraction, in which allowance may be made for difference in the rate of decrease of temperature with the height above the earth's surface at different seasons of the year. M. Radau also discusses the case in which the surfaces of equal temperature in the atmosphere are inclined to the earth's surface.

A new map of the solar spectrum, containing a much larger number of lines than are shown in Ångström's classical normal spectrum, has been published by Professor Vogel in the publications of the new Astrophysical Observatory at Potsdam. In this work Professor Vogel has bestowed great care on estimates of the breadth and intensity of each line. In the same volumes are given the results of Professor Spörer's sun-spot observations at Auclam from 1871 to 1879, in continuation of those for the years 1861 to 1870, previously published. From a comparison of the rotation-angles for 78 spots with the formula, Professor Spörer finds that the larger deviations are always towards the west, indicating that a descending current has brought down with it the larger velocity of the higher regions of the sun's atmosphere. The law previously deduced by Professor Spörer, that, about the time of minimum, spots commence to break out in high latitudes, and that the zone of disturbance gradually approaches the equator till at the next minimum it coincides with it and dies away, to be replaced by a new zone in high latitudes, is confirmed by the recently published Auclam results, comprising (with Carrington's series) two complete spot-cycles.

In astronomical photography an important advance has been made by the successful application of the new processes to the nebulæ as well as to the comets. Professor Henry Draper and Mr. Common have obtained photographs of the great nebula in Orion, showing considerable detail, and Mr. Huggins and Professor Henry Draper have succeeded in photographing its spectrum. Mr. Huggins finds in his photograph a very strong bright line in the ultra-violet at wavelength 3730, in addition to the four nebular lines previously discovered by him in the visible portion. Professor H. Draper's photographs do not show this bright line, though they have faint traces of other lines in the violet, and he thinks that this may be due either to the circumstance that he had placed himself on a different part of the nebula or to his use of a refractor with glass prism, while Mr. Huggins used a reflector and Iceland spar prism. The most striking feature of Professor Draper's photographs is perhaps the discovery of two condensed portions of the nebula (just preceding the Trapezium) which give a continuous spectrum.

Professor Schiaparelli has recently called attention to a peculiar feature on the planet Mars. In 1877 he remarked a number of narrow dark lines, which he called "canals," connecting the dark spots or so-called "seas" of the southern and northern hemispheres. He now finds that these lines are each doubled, so that according to his view the equatorial regions of Mars are covered by a network of pairs of parallel straight lines. It is to be remarked that though the appearance of Mars as depicted by Professor Schiaparelli differs greatly from previous representations, indications of these double "canals" are to be found in the sketches of other observers.

The two bright comets of this year possess more than usual interest. The bright comet discovered at Boston by Wells, on March 18th, was the first comet since the spectroscope was applied to these objects, which presented a spectrum unlike the hydrocarbon type common to all the other comets which appeared since 1864. The eye observations, as well as its photographic spectrum (taken by Mr. Huggins), showed an absence of the hydrocarbon spectrum, which was replaced by a brilliant continuous spectrum and bright lines, including those of sodium.

In September, a very brilliant comet appeared near the sun. It seems to have been discovered independently by Ellery, at Melbourne, Finlay at the Cape, Mr. Common in this country, and also by Thollon and Cruls. This great comet has been a brilliant object in the early mornings during the past two months. On September 17th, an observation, apparently unique in the history of astronomy, was made by Mr. Gill at the Cape, who watched the comet right up to the sun's limb. It could not, however, be detected in the sun, and this circumstance of appearing neither bright nor dark when in front of the sun, appears to suggest a very small substantiality, or great separation of the cometary matter. After perihelion it presented a magnificent appearance, having a tail 30° long, and even on October 30th, the tail covered a space greater than the mean distance of the earth from the sun.

On October 9th, Professor Schmidt discovered a nebulous object not far from the great comet, the orbit of which strongly suggests a connexion in the past with the great comet. This fact is of more interest when the orbits of the great comet of this year, of Comet I, 1880, and of the well-known comet of 1843 are compared. The very near approach of the great comet to the sun will lead astronomers to watch with great interest for its return to our system, whatever may be its destiny, to fall ultimately into the sun, or to disappear through a process of gradual disintegration. In the Astronomische Nachrichten, just published, Professor Pickering (one of whose assistants has computed the elements of the orbit of this comet), states, "I believe the deviation from a parabola to be real, although the corresponding period may be very long. These differences seem to indicate that the disturbance suffered by the comet in passing through the coronal region could not have been great."

This comet presented a spectrum similar to that of Comet Wells, but while receding from the sun, the bright lines of its spectrum became fainter, and then the usual hydrocarbon spectrum made its appearance. This observation, taken in connexion with those of the previous comet, suggests a modified condition of an essentially similar chemical constitution. The phenomena would admit more easily of explanation if the cometary light is supposed to be due to electric discharges, as it is well known how preferential is the electric discharge when several substances are present together in the gaseous form.

Before leaving this subject, I venture to quote the following passage from the "Observatory," which puts in a very clear form the speculations now current, on the relation of the present great comet to that of 1880, 1843, and possibly 1668.

"The physical appearance of the comet, which like that of 1843, and unlike that of 1880, showed at first a decided nucleus, together with the intimation of a period very considerably greater than that of the interval from 1880, January 27, the date of perihelion of the 1880 comet, suggest that perhaps the 1843 comet suffered disintegration when at its nearest approach, and that the 1880 comet was a portion of its less condensed material, whilst the body of the comet with the principal nucleus, suffering less retardation than the separated part, has taken two and a-half years longer to perform a revolution. The remarkable discovery made by Professor Schmidt, of Athens, on October 8, of a second comet only 4° S. W. of the great comet, and having the same motion, would seem to confirm this view."

The scientific year now concluded has not been so fertile as its predecessor in the initiation of great national and international undertakings, neither have any of those larger enterprises which I took occasion to mention last year, such as the circumpolar observations, or the Transit of Venus Expeditions, as yet been brought to their final issue. Nevertheless, in some of them we have evidence that good work is already being done, and in the others, of which we have as yet no information, there is no reason to doubt that the same is the case. Nor again, in the border-land between science proper and its applications, have I to record anything so important as the Paris Electrical Exhibition. That Exhibition, however, bore legitimate fruit in the Electric Lighting Exhibition at the Crystal Palace, and in the technical experiments lately carried out on a large scale at Munich. Perhaps the most prominent feature of the Crystal Palace Show was the incandescent light. At Paris that mode of illumination appeared to be little more than a possibility, in London it had become an accomplished fact. The importance attaching to this advance in electric lighting may be measured both by the rapid extension of its use, and also by the fact that not a few of our leading minds consider that the incandescent lamp is the lamp of the future, not merely for domestic, but even for many other public purposes.

But in another way the present year has witnessed the most important step which could have been taken for the promotion of electric lighting in this country. The Legislature has passed the Electric Lighting Bill, and, so far as legislation can effect the object, it has brought electricity to our doors. Up to this time installations of greater or less magnitude had sprung up sporadically in many parts of the country, in railway stations, manufacturing works, and occasionally in private houses. But, compared with the lighting of a whole town, or even of separate districts of a large city, even the most important of these must be confessed still to partake of the nature of experiments; experiments, it is true, on a large scale, and, as I believe, conclusive as to the ultimate issue. Indeed, by multiplication of machines it is certainly, even now, possible to increase the lighting power to any required extent; but this can hardly be regarded as the final form of solution of the problem, inasmuch as such a method would be as uneconomical as it would be to use a number of small steam-engines instead of a large one. And when we consider that at the time of the passing of the Act in question, there was but one machine actually constructed which was capable of illuminating even one thousand incandescent lamps (I mean that of Edison), we cannot but feel that much remained to be done before the requirements of the public could be fully met. I do not mean thereby to imply that the Act was passed at all too soon; on the contrary, it has already given just that impetus which was necessary for producing installations on a larger scale. In illustration of this, I cannot help mentioning, as the first fruit of the impetus, a remarkable machine, by our countryman Mr. J. E. H. Gordon, which appears capable of feeding from five to six thousand lamps.

But beside the impulse above described, the Bill will have a scientific influence perhaps not contemplated by its original promoters. Under this Act, for the first time in the history of the world, energy will come under the grasp of the law, will become the subject of commercial contracts, and be bought and sold as a commodity of everyday use. It is, in fact, far from improbable that the public supply of electricity will be reckoned and charged for in terms of energy itself. But whether this be literally the case or not, a measurement of energy must lie at the root of every scale of charge.

And, further, since the Act allows no restriction to be placed upon the use of the electricity so supplied, it follows that it may be used, and undoubtedly will be used, at the pleasure and convenience of the customer, either for lighting, or for heating, or for mechanical, or for chemical purposes. This being so, it is clear that the public must by this process become, practically at least, familiar with the various modes of the transformation of force; and the Act in question might, from this point of view, have been entitled An Act for the better Appreciation of the Transformation of Force.

While offering to the public this new commodity electricians may, in one respect, especially congratulate themselves, namely, that their article is incapable of adulteration. An electric current of a given strength and given electro-motive force is perfectly defined, and is identically the same whether it comes from a Siemens or a Gramme, from a magneto- or from a dynamo-machine; or, as was suggested by an eminent counsel before the Select Committee of the House of Commons, just as if it had been merely a question of coming from one machine painted red or from another painted blue.

It has been said, and perhaps with truth, that the electric light will be the light of the rich rather than that of the poor. But in more ways than one electricity may now become the poor man's friend. The advantages in avoidance of heat and of vitiated atmosphere in workshops and factories have often been pointed out, and may ultimately become an important factor in the physical growth and prosperity of our population. But besides this, when electricity is literally brought to our doors, it will become possible, by converting it into motive power of limited extent, to revive some of the small industries which during the last half century have been crushed by the great manufacturing establishments of the country. There are operations which are capable of being carried out by the wives and families of workmen; there are works of small extent which can be performed more advantageously in a small establishment than in a large one, and it can hardly fail to be a gain to the community if this new departure should give fresh opportunities for the development of our industry in these directions.

On the motion of Sir Charles Shadwell, seconded by Dr. Gilbert, it was resolved :—"That the thanks of the Society be returned to the President for his Address, and that he be requested to allow it to be printed."

The President then proceeded to the presentation of the Medals :—

The Copley Medal has been awarded to Professor Arthur Cayley, F.R.S., for his numerous profound and comprehensive researches in Pure Mathematics.

One Royal Medal has been awarded to Professor William Henry Flower, F.R.S. During the last thirty years Professor Flower has been actively engaged in extending our knowledge of Comparative Anatomy and Zoology in general and of the Mammalia in particular.

His Memoirs on the Brain and Dentition of the Marsupialia published in the "Phil. Trans." for 1865 and 1867, established several very important points in morphology, and finally disposed of sundry long-accepted errors.

His paper "On the Value of the Characters of the Base of the Cranium in the Carnivora" (1869), and numerous memoirs on the Cetacea, are hardly less valuable additions to zoological literature.

Professor Flower has been for more than twenty years Curator of the Museum of the Royal College of Surgeons, and it is very largely due to his incessant and well-directed labours that the museum at present contains the most complete, the best ordered, and the most accessible collection of materials for the study of vertebrate structure extant.

The publication of the first volume of the new Osteological Catalogue in 1879, affords an opportunity for the recognition of Professor Flower's services in this direction. It contains carefully verified measurements of between 1,300 and 1,400 human skulls, and renders accessible to every anthropologist a rich mine of cranological data.

The other Royal Medal has been awarded to Lord Rayleigh, M.A., F.R.S.

The researches of Lord Rayleigh have been numerous, and extend over many different subjects; and they are all characterised by a rare combination of experimental skill with mathematical attainments of the highest order.

One class of investigations to which Lord Rayleigh has paid much attention is that of vibrations, both of gases and of elastic solids. The results of most of these researches are now embodied in Lord Rayleigh's important work on the "Theory of Sound"—a work which not only presents the labours of others up to the time of writing in a digested and accessible form, but is full of original matter.

The subject of vibrations naturally leads on to a mention of other hydro-dynamical researches. Lord Rayleigh has investigated the motion of waves of finite height, and in particular has shown that the "great solitary wave" of our late Fellow, Mr. Scott Russell, has a determinate character; and he has investigated the circumstances of its motion to an order of approximation sufficient to apply to waves of considerable height.

Lord Rayleigh has examined more fully than had previously been done the theory of diffraction gratings, and the effects of irregularities; and also investigated the defining power of optical combinations, and its limitation by diffraction and spherical aberration.

He has lately been engaged in the elaborate re-determination of the B.A. unit of electrical resistance.

The Rumford Medal has been awarded to Captain W. de W. Abney, R.E., F.R.S. Captain Abney has contributed largely to the advancement of the theory and practice of photography by numerous investigations. In the Bakerian Lecture for 1880, he has given an account of a method by which photography can be extended to the invisible region below A, which had been hitherto but very imperfectly examined by means of the thermopile.

Making use of plates prepared with silver bromide in a particular molecular condition, Captain Abney, by means of a diffraction grating containing 17,600 lines to the inch, constructed a detailed map of the infra-red region of the solar spectrum extending from A down to λ 10,650 (Plate XXXI, "Phil. Trans.," 1880). The lowest limit of this map was fixed by conditions of the diffraction apparatus, and not by a falling-off of the sensitiveness of the plates at this low point; for, when a prismatic apparatus was used, photographs were obtained which show a continuous spectrum down as far as λ 12,000.

In a subsequent paper ("Phil. Trans.," 1881, p. 887), Captain Abney, working with Lieut.-Col. Festing, R.E., applied this new extension of photography to a research on the influence of the atomic grouping in the molecules of organic bodies on their absorption in the infra-red region of the spectrum. The authors believe that their results indicate, without much doubt, that the complex substances they examined can be grouped according to their absorption spectra, and that such grouping, as far as their experiments go, agrees on the whole with that adopted by chemists. They have more confidence in their results, as they were careful to select such bodies as might be regarded as typical; but, of course, much patient labour of many, for a long period, will be necessary before this new branch of physico-chemical research can be regarded as fully established in any complete form.

Captain Abney has since carried on his work in this new region of the spectrum at different elevations during a recent visit to Switzerland.

The Davy Medal has been awarded to Dimitri Ivanovitch Mendeleeff and Lothar Meyer.

The attention of chemists had for many years past been directed to the relations between the atomic weights of the elements and their respective physical and chemical properties; and a considerable number of remarkable facts had been established by previous workers in this field of inquiry.

The labours of Mendeleeff and Lothar Meyer have generalised and extended our knowledge of those relations, and have laid the foundation of a general system of classification of the elements. They arrange the elements in the empirical order of their atomic weights, beginning with the lightest and proceeding step by step to the heaviest known elementary atom. After hydrogen the first fifteen terms of this series are the following, viz :—

Lithium	7	Sodium	23
Beryllium	9·4	Magnesium........	24
Boron	11	Aluminium........	27·4
Carbon	12	Silicon	28
Nitrogen	14	Phosphorus	31
Oxygen	16	Sulphur	32
Fluorine	19	Chlorine	35·5
		Potassium	39

No one who is acquainted with the most fundamental properties of these elements can fail to recognise the marvellous regularity with which the differences of property, distinguishing each of the first seven terms of this series from the next term, are reproduced in the next seven terms.

Such periodic re-appearance of analogous properties in the series of elements has been graphically illustrated in a very striking manner with respect to their physical properties, such as melting-points and atomic volumes. In the curve which represents the relations of atomic volumes and atomic weights analogous elements occupy very similar positions, and the same thing holds good in a striking manner with respect to the curve representing the relations of melting-points and atomic weights.

Like every great step in our knowledge of the order of nature, this periodic series not only enables us to see clearly much that we could not see before; it also raises new difficulties, and points to many problems which need investigation. It is certainly a most important extension of the science of chemistry.

The Statutes relating to the election of Council and Officers were then read, and Sir Henry Lefroy and Mr. Vaux having been, with the consent of the Society, nominated Scrutators, the votes of the Fellows present were taken, and the following were declared duly elected as Council and Officers for the ensuing year:—

President.—William Spottiswoode, M.A., D.C.L., LL.D.

Treasurer.—John Evans, D.C.L., LL.D.

Secretaries.— { Professor George Gabriel Stokes, M.A., D.C.L., LL.D.
Professor Michael Foster, M.A., M.D.

Foreign Secretary.—Professor Alexander William Williamson, Ph.D., LL.D.

Other Members of the Council.

Professor W. Grylls Adams, M.A.; John Ball, M.A.; Thomas Lauder Brunton, M.D., Sc.D.; Professor Heinrich Debus, Ph.D.; Francis Galton, M.A.; Professor Olaus Henrici, Ph.D.; Professor Thomas Henry Huxley, LL.D.; Professor E. Ray Lankester, M.A.; Professor Joseph Lister, M.D.; Professor Joseph Prestwich, M.A.; Professor Osborne Reynolds, M.A.; Professor Henry Enfield Roscoe, B.A., LL.D.; Marquis of Salisbury, K.G., M.A.; Osbert Salvin, M.A.; Warington W. Smyth, M.A., F.G.S.; Edward James Stone, M.A.

The thanks of the Society were given to the Scrutators.

The following Table shows the progress and present state of the Society with respect to the number of Fellows:—

	Patron and Royal.	Foreign.	Compounders.	£4 yearly.	£3 yearly.	Total.
Nov. 30, 1881 ..	4	50	227	214	39	534
Since Elected ..	+ 1		+ 6	+ 2	+ 11	20
Since Compounded			+ 1	− 1		
Since Deceased ..		− 4	− 12	− 7	− 1	− 24
Since Withdrawn				− 1		− 1
Defaulter ..				− 1		− 1
Nov. 30, 1882 ..	5	46	222	206	49	528

Statement of Receipts and Expenditure from November 26, 1881, to November 24, 1882.

		£	s.	d.
Annual Contributions, 212 at £4	£848	1,001	0	0
,, 51 at £3	153			
Admission Fees		40	0	0
Fee Reduction Fund, in lieu of Admission Fees and Annual Contributions		195	0	0
Compositions		460	0	0
Rents		171	2	7
Dividends (exclusive of Trust Funds)		1,413	13	0
,, on Jodrell Fund		151	15	3
Interest on Mortgage Loan		587	10	0
Sale of Transactions and Proceedings		783	6	6
Donation from J. S. Budgett, Esq.		100	0	0
Sale of Land at Acton		32,207	13	7
Interest on Deposit	£201 1 1 163 15 0	364	16	1
		£37,475	17	0

	£	s.	d.	£	s.	d.
Salaries and Wages				1,061	13	0
The Library Catalogue				80	0	0
Books for the Library				170	16	10
Binding ditto				126	0	2
Printing Transactions, Part III. 1881, Part I. 1882, and Separate Copies to Authors and Publisher	311	18	8	2,005	8	7
Ditto Proceedings, Nos. 215–221	390	17	10			
Ditto Miscellaneous	76	13	9			
Paper for Transactions and Proceedings	295	6	6			
Binding ditto	79	0	3			
Engraving and Lithography	851	11	7			
Soirée and Reception Expenses				69	5	0
Coal, Lighting, &c.	76	12	8	555	3	3
Office Expenses	19	16	8			
House Expenses	240	7	10			
Tea Expenses	19	19	11			
Fire Insurance	41	15	0			
Taxes	33	5	10			
Advertising	16	9	0			
Postage, Parcels, and Petty Charges	31	8	5			
Miscellaneous Expenses	75	7	11			
Law Charges				13	19	8
Law Charges and Expenses, Acton Estate				160	4	2
Purchase of £1,000 Consols				1,003	15	0
,, £5,000 Madras Railway Guaranteed 5 °/₀ Stock				6,565	2	6
,, £5,000 North Eastern Railway 4½ °/₀ Stock, 1876				5,530	0	0
,, £5,000 London and North Western Railway Consolidated Preference Stock				5,593	1	2
Deposit with Bankers				13,217	13	7
,, ,, Few and Co., on Purchase of Ground Rents				1,590	0	0
				£37,742	2	11

Trust Funds.

	£	s.	d.	£	s.	d.
Donation Fund Dividends	417	19	2	615	10	7
Rumford Fund Dividends	68	4	10			
Wintringham Fund ,,	35	5	0			
Copley Medal Fund ,,	58	16	8			
Davy Medal Fund ,,	32	6	2			
Croonian Lecture Fund—Rent from College of Physicians	2	18	9			
Balance at Bank, Dec., 1881	2,241	11	8	2,259	4	7
Do. on hand	17	12	11			
				£40,350	12	2

	£	s.	d.	£	s.	d.
Donation Fund	539	0	0	667	15	0
Davy Medal Fund	32	6	0			
Wintringham Fund	35	2	0			
Copley Medal Fund	54	8	3			
Bakerian Lecture	4	0	0			
Croonian Lecture	2	18	9			
Balance at Bank				1,929	0	0
Balances on hand, Catalogue Account				11	14	3
				£40,350	12	2

JOHN EVANS,
Treasurer.

Estates and Property of the Royal Society, including Trust Fund

Estate at Mablethorpe, Lincolnshire (55 A. 2 R. 2 P.), £110 per annum.
Fee Farm near Lewes, Sussex, rent £19 4*s.* per annum.
One-fifth of the clear rent of an estate at Lambeth Hill, from the College of Physicians, £3 per annum.
Stevenson Bequest. Chancery Dividend. One-fourth annual interest on £85,336, Government Annuities and Bank Stock (produced £509 1*s.* 9*d.* in 1881–82).
£21,000 { £14,952 12*s.* 3*d.* Reduced 3 per Cent. Annuities.
£6,047 7*s.* 9*d.* " " " Handley Fund.
£15,000 Mortgage Loan, 4 per Cent.
£19,314 0*s.* 2*d.* Consolidated Bank Annuities, { being £15,861 19*s.* 1*d.* in name of the R. S., and £3,452 1*s.* 1*d.* in Chancery, arising from sale of the Coleman Street Estate.
£403 9*s.* 8*d.* New 2½ per Cent. Stock—Bakerian and Copley Medal Fund.
£11,511 6*s.* New Threes { £6,328 11s. 2*d.* Scientific Relief Fund.
5,182 14*s.* 10*d.* Jodrell Fund.
£667 5*s.* 6*d.* India Fours.
£660 Madras Guaranteed 5 per Cent. Railway Stock.—Davy Medal Fund.
£10,000 Italian Irrigation Bonds.—The Gassiot Trust.
£1,396 Great Northern Railway 4 per Cent. Debentures—The Trevelyan Bequest.
£100 Metropolitan 3½ per Cent. Stock.—Scientific Relief Fund.
£1,550 " " —Fee Reduction Fund.
£7,000 London and North Western Railway 4 per Cent. Debentures. } " "
Two Hundred Shares in the Whitworth Land Company, Limited. } " "
£5,000 Madras Railway Guaranteed 5 °/₀ Stock.
£5,000 North Eastern Railway 4 °/₀ Stock.
£5,000 London and North Western Consolidated Preference Stock.

We, the Auditors of the Treasurer's Accounts on the part of the Council, have examined these Accounts and found them correct; and we find that the Balance at the Bankers' is £1,929 0*s.* 0*d.*

W. SPOTTISWOODE, *Pres.*
JAS. RISDON BENNETT.
ALEXANDER JOHN ELLIS.
JOSEPH LISTER.
G. G. STOKES, *Sec.*

We, the Auditors of the Treasurer's Accounts on the part of the Society, have examined these Accounts and found them correct; and we find that the Balance at the Bankers' is £1,929 0*s.* 0*d.*

J. T. BOILEAU.
W. H. M. CHRISTIE.
WARREN DE LA RUE.
G. MATTHEY.

Trust Funds. 1882.

Scientific Relief Fund.

	£	s.	d.
New 3 per Cent. Annuities	6,328	11	2
Metropolitan 3½ Consols	100	0	0
	£6,428	11	2

Dr.	£	s.	d.	*Cr.*	£	s.	d.
To Balance	85	2	3	By Grants	80	0	0
„ Dividends	188	16	0	„ Balance	193	18	3
	£273	18	3		£273	18	3

Donation Fund.

£6,339 0*s.* 1*d.* Consols.

The Trevelyan Bequest.

£1,396 Great Northern Railway 4 per Cent. Debentures.

	£	s.	d.		£	s.	d.
To Balance	792	12	1	By Grants	539	0	0
„ Dividends	240	17	8	„ Balance	671	11	3
„ Transferred from Handley Fund	177	1	6				
	£1,210	11	3		£1,210	11	3

Rumford Fund.

£2,322 19*s.* Consols.

	£	*s.*	*d.*		£	*s.*	*d.*
To Balance	67	19	0	By Balance	136	3	10
„ Dividends, 1882	68	4	10				
	£136	3	10		£136	3	10

Bakerian and Copley Medal Fund.

Sir Joseph Copley's Gift, £1,666 13*s.* 4*d.* Consols.
£403 9*s.* 8*d.* New 2½ per Cent.

	£	*s.*	*d.*		£	*s.*	*d.*
To Balance	102	4	1	By Gold Medal	4	8	3
„ Dividends	9	17	6	„ K. A. Wurtz, Sir J. Copley's Gift	50	0	0
„ Dividend—Sir J. Copley's Fund	48	19	2	„ Bakerian Lecture	4	0	0
				„ Balance	102	12	6
	£161	0	9		£161	0	9

The Keck Bequest.

£600 Midland Railway 4 per Cent. Debenture Stock.

	£	*s.*	*d.*		£	*s.*	*d.*
To Dividends, 1882	23	10	0	By Payment to Foreign Secretary	23	10	0

Wintringham Fund.

£1,200 Consols.

	£	*s.*	*d.*		£	*s.*	*d*
To Balance, 1881	35	2	0	By Payment to Foundling Hospital, 1882	35	2	0
„ Dividends, 1882	35	5	0	„ Balance	35	5	0
	£70	7	0		£70	7	0

Croonian Lecture Fund.

	£	s.	d.		£	s.	d.
To Balance, 1881	2	18	9	By Croonian Lecture	2	18	9
,, One-fifth of Rent of Estate at Lambeth Hill, payable by the College of Physicians	2	18	9	,, Balance	2	18	9
	£5	17	6		£5	17	6

Davy Medal Fund.

£660 Madras Guaranteed 5 per Cent. Railway Stock.

	£	s.	d.		£	s.	d.
To Balance	138	18	10	By Gold Medal	32	6	0
,, Dividends	32	6	2	,, Balance	138	19	0
	£171	5	0		£171	5	0

The Gassiot Trust.

£10,000 Italian Irrigation Bonds.
£200 3 per Cent. Consols.

	£	s.	d.		£	s.	d.
To Balance	157	0	9	By Payments to Kew Committee	496	14	6
,, Dividends	502	12	0	,, Balance	162	18	3
	£659	12	9		£659	12	9

Handley Fund.

£6,047 7*s.* 9*d.* Reduced.

	£	*s.*	*d.*		£	*s.*	*d.*
Dividends, 1882	177	1	6	By transferred to Donation Fund	177	1	6

The Jodrell Fund.

£5,182 14*s.* 10*d.* New 3 per Cent. Stock.

	£	*s.*	*d.*		£	*s.*	*d.*
To Dividends, 1882	151	15	3	By transferred to Royal Society General Account	151	15	3

Fee Reduction Fund.

£1,550 Metropolitan Consols $3\frac{1}{2}$ per Cent.

£7,000 London and North Western Railway 4 per Cent. Debentures.

Two Hundred Shares in the Whitworth Land Company, Limited.

	£	*s.*	*d.*		£	*s.*	*d.*
To Balance (1881)	48	18	4	By transferred to Royal Society General Account (1882)	195	0	0
„ Dividends	444	19	1	„ Balance	298	17	5
	£493	17	5		£493	17	5

Account of the appropriation of the sum of £4,000 (the Government Grant) annually voted by Parliament to the Royal Society, to be employed in aiding the Advancement of Science (continued from Vol. XXXIII, p. 76.)

1882.

	£	*s.*	*d.*
A. Macfarlane, for a Quantitative Research on the Conditions of Discharge of Electricity of high Potential......	25	0	0
H. S. Hele Shaw, for the construction of an Improved Anemometer; three diagrams and photograph...........	50	0	0
B. Stewart, for the expense of an Assistant in investigating the Inequalities of Sun-spots, and their Terrestrial Effects..	80	0	0
J. N. Lockyer, for Spectroscopic Researches in connexion with the Spectrum of the Sun.....................	100	0	0
Thos. and Andrew Gray, for continuation of Experiments on the Specific Resistance and Specific Inductive Capacity of different kinds of Glass....................	50	0	0
J. Kerr, for continuation of Experiments in Electro- and Magneto-Optics..	50	0	0
Prof. W. N. Hartley, for continuation of Researches on Ultra-Violet Spectra..................................	150	0	0
H. Tomlinson, for Investigations on the Influence of Stress and Strain on the Action of Physical Forces......	50	0	0
T. Stevenson, for the Reduction and Discussion of Meteorological Observations made from June to October, 1881, at Fort William and on the top of Ben Nevis...........	50	0	0
E. Neison, for continuation of Computations in the Lunar Theory..	50	0	0
Prof. G. D. Liveing, for defraying the cost of Apparatus and Material used by him in Spectroscopic Researches....	200	0	0
C. Michie Smith, for Spectroscope and Apparatus suitable for observing and photographing the Spectrum of the Zodiacal Light..................................	50	0	0
C. Michie Smith, for an Electrometer for observing Atmospheric Electricity................................	20	0	0
A. Mallock, for continuing his experiments on the ruling of large Diffraction-gratings........................	120	0	0
G. F. Rodwell, for the construction of an Apparatus for determining with accuracy the Coefficients of Expansion			
Carried forward............	£1,045	0	0

Brought forward	£1,045	0	0
and Contraction of Bodies at temperatures far exceeding 100° C.	88	10	0
G. R. Vine, for further Investigation of the Morphological Structures of the Organisms found in the Wenlock Shales	25	0	0
Dr. C. Callaway, for continuation of Investigations of the Relation between the newer Gneissic Series of the Highlands and the Fossiliferous Ardovician Group	50	0	0
Rev. O. P. Cambridge, for Investigation, under high Microscopic Power, of the Palpi, Palpal Organs, and other Genital Parts and Processes, external and internal, of Spiders and other Arachnidæ	25	0	0
Prof. W. C. Williamson, for extension of the Research into the Fossil Plants of the Coal-Measures to a Systematic Study of the Microscopic Aspects of the chief Coals from all the Coal-Fields of the World	50	0	0
E. C. Rye, in aid of the Publication Fund of the Zoological Record Association	150	0	0
Dr. R. Braithwaite, for aid in Publishing a Work on the British Moss Flora	50	0	0
G. E. Dobson, for continuation of his illustrated Monograph on the Anatomical Structure, Systematic Position, and Geographical Distribution of the Species of the Order Insectivora	100	0	0
W. Topley (in instalments), for the Preparation and Publication of a Geological Map of Europe, and the adjacent parts of Asia and Africa, under the authority of the International Geological Congress	150	0	0
Dr. J. Hamilton, for continuation of Researches on Topographical Anatomy of the Brain	75	0	0
Dr. Ferrier, for the purchase of Monkeys and other Animals to be used in an Experimental Investigation of some points in the Physiology of the Brain and Spinal Cord	50	0	0
Chas. Roy, for the construction of Apparatus for photographing the Movements of a Lippmann's Galvanometer	35	0	0
Prof. J. Struthers, for the expense of Investigations into the Anatomy of the Greenland Right Whale	25	0	0
Rev. A. E. Eaton, to defray further the cost of printing and publishing a descriptive Monograph of the Ephemeridæ	100	0	0
Carried forward	£2,018	10	0

Brought forward	£2,018	10	0
E. A. Letts, for Materials and Assistance required in Experiments on the Organic Compounds of Phosphorus and Sulphur	40	0	0
L. T. Thorne, for Investigation of the Character and Mode of Formation of an Anhydrous Substance obtained by the Distillation of Ethylacetopropionic Acid	30	0	0
H. B. Dixon, for aid in a Research on the Phenomena of the Combustion of Gases in closed Vessels	150	0	0
Prof. J. S. Humpidge, for the expense of Materials and Apparatus to be employed in the extraction of metallic Glucinum in the compact form, and for Investigations of the Metal, if obtained	50	0	0
C. Schorlemmer, for continuation of researches into (1) Aurin; (2) the Normal Paraffins; (3) Suberone	100	0	0
Profs. Tilden and Shenstone, for assistance in continuing a research into the Constitution of Solutions and the Phenomena of Supersaturation and Superfusion	50	0	0
Dr. B. Brauner, to defray the cost of a Platinum Tube and other Platinum Apparatus for Investigation of the Anhydrous Fluorides by a new Method	30	0	0
Prof. M. F. Heddle, for continuation of a Research connected with the Scientific Mineralogy and Geognosy of Scotland—£100 for analyses, £100 personal	100	0	0
Spencer U. Pickering, for continuation of a Research into Molecular Combinations	50	0	0
W. Saville Kent, for a renewal of former grant to aid him in a further Investigation of the Protozoa and allied Organisms	100	0	0
C. Lapworth, for assistance in Studying the detailed Geology of the Lower Palæozoic Rocks of Britain, and describing the Graptolites they contain	150	0	0
Prof. W. K. Parker, for assistance in his Researches into the Morphology of the Vertebrata, more especially of the Skull	300	0	0
	£3,168	10	0

Dr.	£	*s.*	*d.*	*Cr.*	£	*s.*	*d.*
To Balance on hand, Nov. 30, 1881	1,446	6	3	By Appropriations, as above	3,168	10	0
To Balance of Administrative Expenses	20	1	1	Printing, Postage, Advertising, and other Administrative Expenses	59	10	9
Interest on Deposit	22	7	0	Balance on hand, Nov. 30, 1882	260	13	7
Moiety of Treasury Grant	2,000	0	0				
	£3,488	14	4		£3,488	14	4
Dec. 1, 1882. To Balance, and Moiety receivable from the Treasury	£2,260	13	7				

Account of Grants from the Donation Fund in 1881–82.

Silvanus P. Thompson, for the cost of Experiments in the construction of Polarising Prisms of large aperture (of angle)	£12
D. Mackintosh, for a Systematic Series of Observations in North Wales on the Positions of Boulders, relatively to the Forms of the Natural Surfaces on which they rest, with a view to throw light on the approximate date of the final disappearance of Glaciers and Floating Ice	7
Dr. A. Downes, to study further the Influence of Light on low forms of Life, with especial reference to (1) the Behaviour of such Organisms in various Media; (2) the question of their Destruction, or reduction to a state of dormant Vitality, by Light	10
Profs. Reinold and Rücker, for continuation, with improved Apparatus, of Researches on the Electrical Properties of Thin Films	30
J. N. Langley, for Observations on the Changes which take place in the Cells of the Liver during Secretion; and Observations on the Liver, and on the Gastric Glands of Birds during Digestion	30
E. D. Archibald, for experimental Researches into the Physics of the Atmosphere and its Meteorology by means of Kites	20
Carried forward	£109

Brought forward	£109
R. Etheridge, Jun., and P. H. Carpenter, for aid in the further preparation of their Monograph of the Blastoidea, especially of British species, with their Morphology	30
Dr. De Burgh Birch, for a Microscopical Research into the Growth of Bone	10
A. M. Worthington, for Apparatus for measuring Photographs of Pendent Drops, and for investigating the influence of Electrical Charge on Surface Tension	20
W. T. Dyer, for aid in preparation of an Illustrated Monograph of Cycadæa	20
	£189

Report of the Kew Committee for the Year ending October 31, 1882.

The operations of the Kew Observatory, in the Old Deer Park, Richmond, Surrey, are controlled by the Kew Committee, which is constituted as follows:

General Sir E. Sabine, K.C.B., *Chairman.*

Mr. De La Rue, *Vice-Chairman.*
Capt. W. de W. Abney, R.E.
Prof. W. G. Adams.
Capt. Sir F. Evans, K.C.B.
Prof. G. C. Foster.
Mr. F. Galton.
Vice-Adm. Sir G. H. Richards, C.B.
The Earl of Rosse.
Mr. R. H. Scott.
Lieut.-General W. J. Smythe.
Lieut.-Gen. R. Strachey, C.S.I.
Mr. E. Walker.

The work at the Observatory may be considered under seven heads:—

1st. Magnetic observations.
2nd. Meteorological observations.
3rd. Solar observations.
4th. Experimental, in connexion with any of the above departments.
5th. Verification of instruments.
6th. Aid to other Observatories.
7th. Miscellaneous and financial.

I. MAGNETIC OBSERVATIONS.

The Magnetographs have been in constant operation throughout the year.

In March a new suspension pulley was fitted to the Bifilar magnet in order to reduce the distance between the suspension wires from 6·8 millims. to 5·5 millims., and thus to increase the sensibility of the instrument. This change was recommended by Professor W. G. Adams, in order to make the scale-value about ·0005 millim. mgrm. for 1 millim., as suggested in his Report to the British Association last year. Dr. Wild, of St. Petersburgh, also recommends that all observatories should adopt as far as possible the same uniform scale for their instruments, and suggests that the scale-values should be as follows:—

For the Declination 1 mm. $\delta D=1'$.
„ Bifilar 1 mm. $\delta H=0·0005$ mm. mgr. units.
„ Balance 1 mm. $\delta V=0·0005$ „ „

The following are the values of the ordinates of the various photographic curves as determined at the various dates stated:—

Declination 1 inch$=0^\circ\ 22'·04$. 1 mm.$=0^\circ\ 0'·87$.
Bifilar Jan. 3, 1882, for 1 inch $\delta H=0·0450$ foot grain units.
„ 1 mm. „ $=0·0008$ mm. mgr. units.
„ Mar. 27, „ „ 1 inch „ $=0·0222$ foot grain units.
„ 1 mm. „ $=0·0004$ mm. mgr. units.
Balance Jan. 6, „ „ 1 inch $\delta V=0·0341$ foot grain units.
„ 1 mm. „ $=0·0006$ mm. mgr. units.

The Committee having been asked by the Secretaries of the International Polar Commission to furnish that body with copies of their hourly determinations of the magnetic elements, recommenced the tabulation of the curves which had been suspended in 1879. (See Report for 1880, p. 4.)

With a view, however, of reducing the labour of tabulation, it was decided that a sufficient degree of accuracy and greater rapidity would be obtained by reading the curves by the unassisted eye, without the aid of the tabulating frame and vernier hitherto employed. Scales graduated on glass plates have therefore been prepared, and the curves tabulated from August 1st up to the present date by this means; the declination being recorded to a tenth of a minute of arc, and the force-traces to the tenth of a millimetre.

In order to obtain a record of the more rapid changes which take place during magnetic storms, a trial has been in progress since July 4 of the highly sensitive argentic gelatino-bromide photographic paper prepared by Messrs. Morgan and Kidd.

The results of the experiment show that the paper indicates clearly small movements of the magnet which the waxed paper is unable to register, and also that less gas-light is needed for the purpose of illumination.

Three magnetic storms, or periods of considerable disturbance of the needles, have been registered during the year; viz., on April 17th and 20th and on October 2nd. All were accompanied by auroral displays, but these were only observed in this country on the last date.

The Committee have to acknowledge with thanks the receipt of photographic copies of traces during those magnetic disturbances from the Observatories at the Mauritius, Melbourne, Toronto, and Batavia.

The monthly observations with the absolute instruments have been made regularly, and the results are given in the tables forming Appendix I of this Report.

The magnetic instruments have been studied, and a knowledge of their manipulation obtained by—

M. Puiseux.
Captain Dawson, R.A., and 3 of his assistants.
Dr. Ristori.
Mr. Dallas.

Information on matters relating to terrestrial magnetism and various data have been supplied to Professor W. G. Adams, J. E. H. Gordon, Dr. Stewart, Messrs. Tate, Zambra, Professor McLeod, The Hydrographic Department of the Admiralty, the Director-General of the Chart Depôt of the French Marine, Lieutenant Chadwick, the Naval Attaché from the United States, and others.

The following is a summary of the number of magnetic observations made during the year:—

Determinations of	Horizontal Intensity	33
,,	Dip	138
,,	Absolute Declination	28

At the request of the Polar Committee of the Royal Society a number of old magnetic instruments were removed out of store, and after repair, packed and delivered to Captain Dawson, R.A., who has been intrusted by the Government with the charge of a temporary observatory established in connexion with the International system at Fort Rae, Great Slave Lake, N.W. America.

Other instruments were lent to the Rev. S. J. Perry, F.R.S., for use during their residence in Madagascar for the observation of the transit of Venus by a party under his direction; and a third set were prepared for Dr. Ristori, who projected an expedition to Iceland, but has not yet started for that country. (See Appendix III.)

A Dip-circle was also lent to the Austrian expedition to Jan Mayen, to replace one mislaid at the time of sailing of the vessels; this, however, having been recovered by the expedition, the Kew circle has been returned.

II. METEOROLOGICAL OBSERVATIONS.

The several self-recording instruments for the continuous registration of atmospheric pressure, temperature, and humidity, wind (direction and velocity), sunshine, and rain, respectively, have been maintained in regular operation throughout the year.

The tube of the wet bulb thermograph was accidentally broken on June 30 by a workman engaged in painting the exterior of the building. A spare tube was substituted for it, and only a few hours' trace lost. The scale value of the curves has been altered, and new tabulating scales are accordingly being constructed at the Meteorological Office.

The standard eye observations made five times daily, for the control of the automatic records, have been duly registered through the year, together with the additional daily observations at 0 h. 8 m. P.M. in connexion with the Washington synchronous system.

The tabulation of the meteorological traces has been regularly carried on, and copies of these, as well as of the eye observations, with notes of weather, cloud, and sunshine have been transmitted weekly to the Meteorological Office.

The following is a summary of the number of meteorological observations made during the past year:—

Readings of standard barometer	1929
,, dry and wet thermometers	4358
,, maximum and minimum thermometers	930
,, radiation thermometers	706
,, rain gauges	730
Cloud and weather observations	1929
Measurements of barograph curves	9125
,, dry bulb thermograph curves	9125
,, wet bulb thermograph curves	6850
,, wind (direction and velocity)	17480
,, rainfall curves	809
,, sunshine traces	2262

In compliance with a request made by the Meteorological Council to the Kew Committee, the Observatories at Aberdeen, Armagh, Falmouth, Glasgow, Oxford (Radcliffe), Stonyhurst, and Valencia, have been visited as on former occasions, and their instruments inspected by Mr. Whipple during his vacation.

With the concurrence of the Meteorological Council, weekly abstracts of the meteorological results have been regularly forwarded to, and published by "The Times," "The Illustrated London News," "The Torquay Directory," and "The Torquay Standard," and data have been supplied to the editor of "Symons's Monthly Meteorological Magazine," the Secretary of the Institute of Mining Engineers, Messrs. Gee, Greaves, Gwilliam, Mawley, Rowland, and others.

Electrograph.—This instrument has been in continuous action through the year.

In August it was dismounted, and a fresh supply of acid placed in the jar, the charge-keeping properties of which had become slightly deteriorated.

With a view of investigating the effect of locality upon the indications of the electrograph, a Thomson's portable electrometer has been employed, with a burning-match collector to make occasional observations around the exterior of the building. These observations are at present suspended, on account of an accidental derangement of the instrument which has necessitated its return for a time to the hands of the maker.

The curves have been tabulated up to the end of 1881, and a report on the working of the instrument has been submitted to the Meteorological Council.

Mr. W. L. Dallas of the Meteorological Office, having recently been appointed Scientific Assistant to the Meteorological Reporter of India, received instructions in the use of meteorological instruments prior to his departure to that country.

III. SOLAR OBSERVATIONS.

The only solar work done at Kew during the past year has been the regular maintenance of the eye observations of the sun, after the method of Hofrath Schwabe, as described in the Report for 1872, in order to preserve the continuity of the Kew records of sun-spots. These have been made on 197 days. The sun's surface was found to be free from spots on three of those days.

A small portable $2\frac{3}{4}$ inch refracting telescope, with a magnifying power of 42 diameters, was used by the observer till July 3rd, since that date the observations have been made by means of the Photoheliograph, which was removed from the Loan Collection at South Kensington for that purpose, and reinstated on the pedestal in the Dome, a position which it occupied prior to its being sent to the Royal Observatory, Greenwich, in 1873.

The spots are now drawn by the Observer, as they appear projected upon the focussing screen.

The measurements and reductions of sun-spot positions, as determined by means of the Kew photoheliograph, from 1864 to 1872, having been completed for Mr. De La Rue, he has deposited the manuscript with the Council of the Royal Society. The correction of area measurements, for foreshortening, still remains to be applied to the reductions for the last two years, but this work is now being rapidly pushed forward.

Transit Observations.—One hundred and twelve observations have been made of sun-transits, for the purpose of obtaining correct local time at the Observatory; 168 clock and chronometer comparisons have also been made.

Shelton's Clock, K.O., has been fitted up in the pendulum room, in a convenient position for observing, and a hearing tube led to the side of the transit instrument, so that its errors may be determined without the intervention of a chronometer. It has accordingly been made the standard timepiece of the Observatory, instead of Shelton R.S. No. 35 fixed in the computing room, which has hitherto been so employed.

A redetermination has been made of the value of the scale divisions of the level of the transit instrument.

The De La Rue Micrometer has been recently employed by Dr. Schuster in the measurement of his photographs of the comet observed during the eclipse of last May.

IV. EXPERIMENTAL WORK.

Exposure of Thermometers.—The observations, made on the lawn of the Observatory, with the view of determining the relative merits of different patterns of thermometer screens were discontinued in November, 1881, the Wild's screen and the De La Rue portable screen being dismounted and returned to the Meteorological Office. The Stevenson's screen was, however, purchased by the Committee, and remains standing *in situ* for the purpose of exhibition to visitors, and also in order that occasional thermometric experiments may be conducted in it.

An exhaustive discussion of the twenty-eight months' observations has been made by the Superintendent, and submitted to the Meteorological Council, at their request.

It may, however, be stated here, that the results show that the observations of air temperature in the thermograph screen, attached to the Observatory building, only differ in the daily mean from those in a freely exposed Stevenson screen 4 feet above the ground by 0°·4, and from a similarly placed Wild's screen, 10 feet above the surface, by 0°·1. The extreme variations observed have, however, occasionally reached several degrees.

Glycerine Barometer.—This instrument, although still standing in the Library, has not been read since December last. No results having as yet been published of the comparisons made for Mr. Jordan, the inventor, the Committee are unable to form any opinion of the scientific value of the instrument.

Pendulum Experiments.—The pendulum operations in progress at the date of the last Report were terminated in November, 1881, by Major Herschel, R.E., and the instruments he employed (see Appendix III, p. 24) were conveyed by him, first to the Royal Observatory, Greenwich, and subsequently to a house near Portland Place, London. Series of observations were made in both those places, and it is hoped that by this means data will have been obtained, which will serve to reduce to a common standard the determinations of gravity made by Kater, Airy, Sabine, and others.

On the conclusion of these experiments, Major Herschel conveyed the pendulums, clock, &c., to America, where, after making a series of observations at Washington, he handed them over to the officers of the United States' Coast Survey Department, in whose charge they now remain.

Actinometry.—At the request of the Meteorological Council, the actinometer devised by Professor Balfour Stewart, and described in the Report of the Committee on Solar Physics, 1880, Appendix H, has been obtained on loan from South Kensington, and erected on a suitable stand on the Observatory Lawn. Numerous observations of solar radiation have been made by it during the past summer, and also several comparative observations have been made with the Hodgkinson's Actinometers belonging to the Royal Society and to the India Office.

At the request and cost of the Indian Government, Sergeant Rowland, R.E., who has since proceeded to India with a view of observing, by means of Stewart's instrument, the solar intensity at Leh, for a period of three years, has received special instruction in the use of these actinometers.

The Committee have had under consideration the desirability of continuing the observations on the actinic power of daylight, which ceased in November, 1875, on account of the unsatisfactory performance of the first photometer constructed. The instrument being now made in the improved form suggested by Captain Abney, R.E., is not liable to the derangements experienced by that formerly employed.

Rating of Chronometers and Watches.—The Superintendent having, from time to time, been requested to certify as to the going of chronometers, has been in communication with the Directors of the Observatories at Bidston, Geneva, Neuchâtel, and Yale, where arrangements exist for the testing and rating of chronometers and superior watches.

The Committee, after receiving his reports upon the subject and also a favourable expression of opinion from the British Horological Institute, considered, however, that the funds at their disposal were insufficient for the present to allow them to extend their operations in this direction.

Water-surface Temperature.—At the request of Mr. C. Greaves, C.E., several series of observations were taken at frequent intervals during last summer of the temperature of the surface of the pond, a quarter of a mile distant from the Observatory. More recently a float has been moored in the centre carrying maximum and minimum thermometers immersed just below the water-line. This is hauled to the shore every morning at 9 A.M., and the temperatures recorded. The cost of the experiment is defrayed by Mr. Greaves.

Nocturnal Radiation.—Professor Tyndall having suggested the desirability of making a series of experiments on the fall of temperature near the surface of the ground at the time of sunset, a scheme was organised and apparatus devised by Mr. F. Galton, by means of which thermometers suspended at heights of 2 feet, 4 feet, and 20 feet could be rapidly read and their indications compared with those of a thermometer placed on swans' down on the surface of the ground.

The apparatus employed is conveyed into the open park to some distance from any building or trees, and the thermometers read at five-minute intervals from about half-an-hour before sunset until one or two hours after.

The cost of these experiments will be defrayed by a grant from the Meteorological Council.

V. VERIFICATION OF INSTRUMENTS.

The following magnetic instruments have been verified, and their constants have been determined :—

1 Unifilar Magnetometer for Negretti and Zambra.
2 Unifilar Magnetometers for Elliott Brothers.
2 Dip Circles for Elliott Brothers.
1 Dip Circle for Casella.

There have also been purchased on commission and verified :—

A Unifilar Magnetometer for the Toronto Observatory.
A Unifilar Magnetometer for the Zi Ka Wei Observatory, China.
2 Dip Circles, with tripod stands, for Dr. Neumayer, Hamburg.
1 Dip Circle, with tripod stand, for Professor Brioschi, Naples.
1 ,, ,, for M. Snellen, Utrecht.
1 ,, ,, for Dr. Hann, Vienna.
1 ,, ,, for Dr. Wild, St. Petersburg.
1 ,, ,, for Professor Nordenskiold, Helsingfors.
A Vertical Force Needle for Dr. Viegas, Coimbra.
A Deflection Bar and Pair of Magnetizing Bars for Dr. Rijkevorsel, Rotterdam.
A Pair of Dip-circle Agates for Senhor Capello, Lisbon.

The number of meteorological instruments verified continues still to increase, having been in the past year as follows :—

Barometers, Standard	48
,, Marine and Station	105
Aneroids	30
Total	183

Thermometers,	ordinary Meteorological	1518
,,	Standard	166
,,	Mountain	69
,,	Clinical	5365
,,	Solar radiation	143
	Total	7261

Besides these, 27 Deep-sea Thermometers have been tested, 2 of which were subjected in the hydraulic press, without injury, to pressures exceeding three and a half tons on the square inch, and 73 Thermometers have been compared at the freezing-point of mercury, making a total of 7361 for the year.

Duplicate copies of corrections have been supplied in 145 cases.

Eleven Standard Thermometers have also been calibrated and divided, and supplied to societies and individuals during the year.

The following miscellaneous instruments have also been verified :—

Hydrometers	195
Anemometers	12
Rain Gauges	4
Theodolites	4
Sextants	36
Index Glasses for ditto, unmounted	2
Horizon ,, ,, ,,	2
Prismatic Compasses	4

There are at present in the Observatory undergoing verification, 7 Barometers, 160 Thermometers, 10 Anemometers and 7 Sextants.

A Barograph and Thermograph have been examined, and had their scale values determined for the Government Astronomer, Adelaide, South Australia; and a Standard Barometer has also been compared for Professor Tacchini, of the Italian Meteorological Service.

A Redier Barograph, purchased by Mr. Dowson at the suggestion of the Superintendent, was put up at the Observatory, and its performance tested for a fortnight before being forwarded to him.

Dr. Siemens having placed one of his Electrical Thermometers at the disposal of the Meteorological Society for their observations of the temperature at the summit of Boston Church Tower, 270 feet high, this instrument was tested for a few days at the Observatory and found to work satisfactorily.

Sextant-testing.—A report upon the errors of Sextants, based upon the comparisons made at the Observatory since the introduction of the present system in 1865, has been submitted to Mr. Galton, at his request.

With a view of checking the values given by means of the Cooke Collimators, a series of angles subtended by various distant well-defined objects at a point at the Observatory, have been carefully determined.

The number of surveying instruments tested has satisfactorily increased during the past year.

Standard Barometers.—From time to time comparisons have been made between the two Welsh Standard Barometers and Newman No. 34, the working Standard of the Observatory, and their relative values have been found to remain unchanged.

Standard Thermometers.—Dr. Waldo, Director of the Thermometric Bureau of the Winchester Observatory, United States of America, has visited the Observatory, and selected several standard thermometers for use in that establishment in the verification of American thermometers, and for comparison with other instruments purchased of Continental makers.

Experiments have been made, but hitherto without complete success, for the direct comparison of chemical thermometers at high temperatures, an operation for which a demand has recently arisen amongst those who supply these instruments in commerce.

VI. Aid to Observatories.

Waxed Papers, &c., supplied.—Waxed paper has been supplied to the following Observatories:—

Coimbra, Vienna, Valencia, Colaba, Batavia, and to the Meteorological Office.

Photographic Material, &c., has been also procured for, and transmitted to, the Coimbra Observatory.

Anemograph Sheets have been sent to the Coimbra Observatory, and *Blank Magnetic Observation Forms* have been supplied to

Mr. W. N. Shaw, Cavendish Laboratory;
Professor Mohn, Christiania;
Dr. Lodge, Liverpool Science College;
Captain Dawson, R.A., Circumpolar Expedition;
The Toronto Observatory;

and to Messrs. Casella, Elliott Brothers, and Negretti and Zambra.

At the request of the Crown Agents for the Colonies, a copy of the apparatus used at Kew for measuring the areas of sun-spots has been procured for the Mauritius Observatory.

A Standard Barometer has also been obtained for the same Observatory.

A request has been received from the Director of the Lisbon Observatory for an Electrograph similar to that employed in the Observatory. The instrument is now in course of construction.

In accordance with instructions received from the Council of the Royal Society, ten volumes of miscellaneous registers, principally of magnetic observations made at Toronto during the years 1840–49, which were deposited in the Magnetic Office of General Sir E. Sabine, in the Observatory, have been returned to Canada, in order that they may be utilised by Mr. Carpmael, the Director of the Toronto Observatory.

Particulars as to the method employed for testing sextants at Kew have been forwarded at his request to Dr. G. Neumayer, Director of the Deutsche Seewarte, Hamburg.

VII. MISCELLANEOUS AND FINANCIAL.

Tenure of the Observatory.—In January last an inquiry was instituted by Her Majesty's Commissioners of Works and Public Buildings as to the conditions under which the President and Council of the Royal Society occupied the Observatory building, and it was discovered that through inadvertence no intimation had been made, in 1872, to their office of the transfer of the building from the British Association to the Royal Society.

Steps were immediately taken to rectify the omission, and in May Mr. Mitford, Secretary to the Office, informed the Secretary of the Royal Society that Her Majesty's sanction had been obtained for the continuance of the occupation of the Royal Observatory at Kew by the Royal Society upon the following conditions:—

1st. The occupation shall be only during the pleasure of Her Majesty and of the Department.

2nd. The internal repairs, painting, papering, and whitewashing shall be done by the tenants once at least in every seven years, the external works being executed by the Department.

3rd. No structural alteration shall be effected without the consent of the Board.

The above conditions were submitted by the President and Council to the Kew Committee, who have agreed to the terms laid down.

The Secretary of State for the Colonies having consulted the Committee as to the equipment of the new Observatory at Hong Kong, has been advised by them as to the instruments they would recommend as desirable for use at that Institution.

The Committee have also recommended the establishment at the Royal Observatory at the Cape of Good Hope of a set of self-recording magnetographs.

Complete specimen sets of curves from the various photographic and autographic instruments in use at the Observatory have been prepared and forwarded to the exhibitions of the Society of Arts, London, and the Royal Cornwall Polytechnic Society, Falmouth.

A number of anemometers and other instruments of interest were also exhibited at the Anemometrical Exhibition of the Meteorological Society, held in the rooms of the Institution of Civil Engineers in March.

By the consent of the Committee the Superintendent, in conjunction with Mr. Baker, submitted the following paper to the Meteorological Society, which has been published in the Quarterly Journal (Vol. VIII, p. 198):—

Barometric gradients in connexion with wind velocity and direction at the Kew Observatory.

Library.—During the year the Library has received, as presents, the publications of

26 English Scientific Societies and Institutions, and

91 Foreign and Colonial Scientific Societies and Institutions.

Observatory and Grounds.—The buildings and grounds have been kept in order throughout the year, and portions of the exterior as well as the interior have been painted by Her Majesty's Commissioners of Works, &c. They have also fitted stoves in the Superintendent's room and Library, and re-covered with sheet zinc the roof of the sun-room.

The footpath and entrance to the Old Deer Park still remain in an unsatisfactory condition, no action having been taken by Her Majesty's Commissioners of Woods and Forests in the matter.

PERSONAL ESTABLISHMENT.

No changes having taken place during the year,
The staff employed now is as follows:—

G. M. Whipple, B.Sc., Superintendent.
T. W. Baker, First Assistant.
J. Foster, Verification Department.
H. McLaughlin, Librarian and Accountant.
F. G. Figg, Magnetic Observer.
E. G. Constable, Solar Observations and Tabulation of Meteorological Curves.
T. Gunter } Verification Department.
C. Taylor }
W. Boxall, Photography.
E. Dagwell, Office duties.
J. Dawson, Messenger and Care-taker.

With the view of exhibiting the financial position of the Observatory during the first decade of its operations under the present Committee, a summarised statement is appended of the receipts and disbursements during the ten years 1871–1881. (Appendix IV.)

An appendix is also given showing what instruments belonging to the Observatory are out of the custody of the Superintendent on loan, at the present time.

Abstract. *Kew Observatory Receipts and Payments Account from November 2, 1881, to November 4, 1882.*

Dr. RECEIPTS.

	£	s.	d.	£	s.	d.
To Balance from 1880–81				414	1	8
Royal Society (Gassiot Trust)				496	14	6
Meteorological Office				400	0	0
Meteorological Office, for Researches, Postages, &c.				35	11	7
Verification Fees, Meteorological Office	£97	8	0			
,, ,, Observatories and Institutions	53	2	0			
,, ,, Instrument Makers and others	471	7	0			
				621	17	0
Payment for Instruments on Commission				516	10	11
Sale of Waxed Paper				34	5	0
,, Photographic Residues				4	7	11
,, Standard Thermometers	26	15	0			
,, Forms, &c.	24	7	3			
Payments for Copying Registers	65	2	1			
				116	4	4
Mr. Jordan, for Glycerine Barometer Experiments	1	9	0			
Mr. Greaves, for Water Surface Temperature Observations	7	17	10			
				9	6	10
				£2648	19	9

PAYMENTS. *Cr.*

	£	s.	d.	£	s.	d.
By Salaries	£1034	10	10			
Extra Payments	156	7	4			
				1190	18	2
Fuel and Gas	77	10	4			
Furniture and Fittings	16	15	3			
Chandlery, &c.	10	6	9			
Painting and Repairs	26	4	11			
Rent and Maintenance of Enclosure and Path	25	14	1			
				156	11	4
Printing and Stationery (General)	30	15	6			
,, ,, (Verification Department)	24	12	6			
Postages	16	3	1			
Library	19	0	7			
Messenger and Housekeeper	62	8	0			
Advertisements	3	5	4			
Porterage and Contingencies	19	13	10			
				175	18	10
Purchase of Chemicals and Materials	17	8	0			
,, ,, Tubes for Standard Thermometers	1	0	10			
,, ,, Anemograph Sheets	1	8	4			
Verification Department Expenses (Ice, Carbonic-Acid Gas, &c.)	15	1	3			
Repair of Instruments, and Purchase of New	31	15	8			
Carpenter's Work and Sundries	5	15	7			
				72	9	8
Postages and Payments on behalf of Meteorological Office				33	4	0
Instruments purchased on Commission				449	5	7
Purchase of Waxed Paper, Packing ditto, &c.				46	11	4
Payments on behalf of Experimental Work				19	5	10
Balance—Bank of England	415	11	9			
London and County Bank	70	0	0			
Cash in hand	19	3	3			
				504	15	0
				£2648	19	9

November 15, 1882. Examined and compared with the Vouchers, and found correct. (Signed) GEO. HENRY RICHARDS, *Auditor.*

ASSETS.

	£	s.	d.
By Balance as per Statement	504	15	0
Meteorological Office, Allowances and Sundries	38	6	4
Commissions	35	0	0
Waxed Paper	43	0	0
Blank Forms	7	17	10
Verification Fees due	45	10	5
Standard Thermometers	83	6	6
Experimental and Miscellaneous	20	10	0
	£778	6	1

LIABILITIES.

	£	s.	d.
To Gas, Fuel, and House Account	11	12	3
Apparatus, Chemicals, &c.	6	3	9
Commissions	34	15	0
Balance	725	15	1
	£778	6	1

November 17, 1882. (Signed) G. M. WHIPPLE.

APPENDIX I.

Magnetic Observations made at the Kew Observatory, Lat. 51° 28′ 6″ *N. Long.* $0^h\ 1^m\ 15^s{\cdot}1$ *W., for the year October* 1881 *to September* 1882.

The observations of Deflection and Vibration given in the annexed Tables were all made with the Collimator Magnet marked K C 1, and the Kew 9-inch Unifilar Magnetometer by Jones.

The Declination observations have also been made with the same Magnetometer, Collimator Magnets N D and N E being employed for the purpose.

The Dip observations were made with Dip-circle Barrow No. 33, the needles 1 and 2 only being used; these are $3\frac{1}{2}$ inches in length.

The results of the observations of Deflection and Vibration give the values of the Horizontal Force, which, being combined with the Dip observations, furnish the Vertical and Total Forces.

These are expressed in both English and metrical scales—the unit in the first being one foot, one second of mean solar time, and one grain; and in the other one millimetre, one second of time, and one milligramme, the factor for reducing the English to metric values being 0·46108.

By request, the corresponding values in C.G.S. measure are also given.

The value of log π^2K employed in the reduction is 1·64365 at temperature 60° F.

The induction-coefficient μ is 0·000194.

The correction of the magnetic power for temperature t_0 to an adopted standard temperature of 35° F. is

$$0{\cdot}0001194(t_0-35)+0{\cdot}000,000,213(t_0-35)^2.$$

The true distances between the centres of the deflecting and deflected magnets, when the former is placed at the divisions of the deflection-bar marked 1·0 foot and 1·3 feet, are 1·000075 feet and 1·300097 feet respectively.

The times of vibration given in the Table are each derived from the mean of 12 or 14 observations of the time occupied by the magnet in making 100 vibrations, corrections being applied for the torsion-force of the suspension-thread subsequently.

No corrections have been made for rate of chronometer or arc of vibration, these being always very small.

The value of the constant P, employed in the formula of reduction $\frac{m}{X}=\frac{m'}{X'}\left(1-\frac{P}{r_0^2}\right)$, is $-0{\cdot}00109$.

In each observation of absolute Declination the instrumental readings have been referred to marks made upon the stone obelisk erected 1,250 feet north of the Observatory as a meridian mark, the orientation of which, with respect to the Magnetometer, was determined by the late Mr. Welsh, and has since been carefully verified.

The observations have all been made and reduced by Mr. F. G. Figg.

Observations of Deflection for Absolute Measure of Horizontal Force.

Month.	G. M. T.	Distances of Centres of Magnets.	Temperature.	Observed Deflection.	Log $\frac{m}{X}$. Mean.
1881.	d. h. m.	foot.	°	° ′ ″	
October........	26 12 30 P.M.	1·0	51·5	15 28 52	9·12639
		1·3	....	6 58 55	
	2 35 „	1·0	51·7	15 27 12	
		1·3	....	6 58 3	
November......	25 12 37 P.M.	1·0	55·6	15 27 0	9·12613
		1·3	....	6 58 5	
	2 30 „	1·0	54·7	15 26 46	
		1·3	....	6 57 58	
December	23 12 32 P.M.	1·0	32·1	15 29 59	9·12600
		1·3	....	6 59 26	
	2 19 „	1·0	34·9	15 29 15	
		1·3	....	6 59 14	
1882. January........	26 12 30 P.M.	1·0	35·0	15 29 31	9·12591
		1·3	....	6 59 19	
	2 32 „	1·0	35·9	15 28 40	
		1·3	....	6 58 57	
February	28 12 27 P.M.	1·0	49·1	15 26 30	9·12548
		1·3	....	6 57 51	
	2 40 „	1·0	51·5	15 25 54	
		1·3	....	6 57 33	
March	24 12 34 P.M.	1·0	57·7	15 27 0	9·12608
		1·3	....	6 58 3	
	2 56 „	1·0	58·2	15 25 41	
		1·3	....	6 57 31	
April..........	25 12 43 P.M.	1·0	57·2	15 27 17	9·12632
		1·3	....	6 58 26	
	2 50 „	1·0	54·2	15 26 43	
		1·3	....	6 58 7	
May	26 12 34 P.M.	1·0	68·5	15 23 51	9·12558
		1·3	....	6 56 51	
	2 51 „	1·0	66·5	15 23 23	
		1·3	....	6 56 40	
June	27 12 34 P.M.	1·0	71·7	15 25 18	9·12627
		1·3	....	6 57 28	
	2 35 „	1·0	73·8	15 23 35	
		1·3	....	6 56 31	
July	26 12 29 P.M.	1·0	67·7	15 24 6	9·12560
		1·3	....	6 57 0	
	2 35 „	1·0	69·6	15 22 57	
		1·3	....	6 56 20	
August	30 12 48 P.M.	1·0	66·8	15 23 50	9·12539
		1·3	....	6 56 48	
	2 51 „	1·0	68·7	15 22 41	
		1·3	....	6 56 12	
September......	27 12 31 P.M.	1·0	59·6	15 25 22	9·12553
		1·3	....	6 57 24	
	2 32 „	1·0	57·8	15 24 30	
		1·3	....	6 57 5	

Vibration Observations for Absolute Measure of Horizontal Force.

Month.	G. M. T.	Temperature.	Time of one Vibration.*	Log mX. Mean.	Value of m.†
1881.	d. h. m.		secs.		
October..........	26 11 54 A.M.	50°·8	4·6483		
	3 13 P.M.	51·9	4·6465	0·30915	0·52212
November........	25 11 51 A.M.	54·3	4·6510		
	3 4 P.M.	54·1	4·6502	0·30883	0·52177
December........	23 11 53 A.M.	30·2	4·6438		
	2 50 P.M.	36·4	4·6448	0·30876	0·52165
1882.					
January..........	26 11 50 A.M.	33·1	4·6427		
	3 3 P.M.	36·0	4·6425	0·30918	0·52184
February	28 11 50 A.M.	47·9	4·6468		
	3 11 P.M.	51·9	4·6499	0·30903	0·52149
March...........	24 11 55 A.M.	56·3	4·6536		
	3 34 P.M.	56·6	4·6495	0·30880	0·52172
April............	25 12 4 P.M.	57·6	4·6540		
	3 36 P.M.	52·7	4·6485	0·30877	0·52184
May.............	26 11 59 A.M.	68·2	4·6557		
	3 27 P.M.	68·3	4·6548	0·30885	0·52145
June	27 11 48 A.M.	71·5	4·6579		
	3 13 P.M.	74·8	4·6578	0·30866	0·52176
July.............	26 11 54 A.M.	67·4	4·6535		
	3 12 P.M.	70·1	4·6535	0·30918	0·52166
August	30 12 14 P.M.	65·5	4·6551		
	3 24 P.M.	69·6	4·6526	0·30904	0·52145
September........	27 11 49 A.M.	59·7	4·6550		
	3 19 P.M.	60·1	4·6513	0·30870	0·52133

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* A vibration is a movement of the magnet from a position of maximum displacement on one side of the meridian to a corresponding position on the other side.

† m=magnetic moment of vibrating magnet.

Dip Observations.

Month.	G. M. T.	Needle.	Dip.	Month.	G. M. T.	Needle.	Dip.
			North.				North.
1881.	d. h. m.	No.	° ′	1882.	d. h. m.	No.	° ′
Oct.	28 3 10 P.M.	1	67 41·31	April	26 3 32 P.M.	1	67 40·56
	3 10 „	2	41·31		3 32 „	2	40·40
	31 3 15 „	1	42·43		27 3 43 „	1	40·81
	3 15 „	2	42·62		3 43 „	2	41·50
	Mean..	....	67 41·92		Mean..	....	67 40·82
Nov.	29 3 7 P.M.	1	67 43·12	May	25 3 27 P.M.	1	67 39·43
	3 7 „	2	43·34		3 25 „	2	40·18
	30 2 59 „	1	42·43		30 3 20 „	1	41·31
	2 58 „	2	42·99		3 19 „	2	41·43
	Mean..	....	67 42·97		Mean..	....	67 40·59
Dec.	29 3 6 P.M.	1	67 41·25	June	26 3 13 P.M.	1	67 42·65
	3 6 „	2	41·81		3 14 „	2	42·56
	30 3 11 „	1	41·93		29 3 6 „	1	41·12
	3 13 „	2	41·87		3 3 „	2	40·78
	Mean..	....	67 41·71		Mean..	....	67 41·78
1882. Jan.	30 3 4 P.M.	1	67 41·50	July	28 3 3 P.M.	1	67 39·71
	3 4 „	2	41·37		3 3 „	2	38·31
	31 3 16 „	1	41·62		31 3 5 „	1	39·81
	3 15 „	2	41·25		3 5 „	2	39·56
	Mean..	....	67 41·43		Mean..	....	67 39·35
Feb.	21 3 10 P.M.	1	67 41·25	Aug.	26 3 23 P.M.	1	67 41·34
	3 11 „	2	42·12		3 23 „	2	39·96
	23 3 20 „	1	40·68		28 3 49 „	1	41·37
	3 19 „	2	41·06		3 50 „	2	39·74
	Mean..	...	67 41·28		Mean..	....	67 40·60
Mar.	28 3 11 P.M.	1	67 40·87	Sept.	28 3 13 P.M.	1	67 40·24
	3 10 „	2	41·31		3 14 „	2	39·68
	29 3 14 „	1	41·31		29 3 14 „	1	40·81
	3 15 „	2	40·62		3 13 „	2	40·12
	Mean..	....	67 41·03		Mean..	....	67 40·21

Month.	Declination.	Magnetic Intensity.								
		English Units.			Metric Units.			C. G. S. Units.		
	Mean of Observations.	X, or Horizontal Force.	Y, or Vertical Force.	Total Force.	X, or Horizontal Force.	Y, or Vertical Force.	Total Force.	X, or Horizontal Force.	Y, or Vertical Force.	Total Force.
	West.									
1881. October	18° 47′ 17″	3·9028	9·5155	10·2847	1·7995	4·3874	4·7421	0·1799	0·4387	0·4742
November	18 48 52	3·9026	9·5231	10·2918	1·7994	4·3910	4·7454	0·1799	0·4391	0·4745
December	18 47 12	3·9028	9·5139	10·2832	1·7995	4·3867	4·7414	0·1799	0·4387	0·4741
1882. January	18 43 28	3·9051	9·5170	10·2870	1·8006	4·3881	4·7432	0·1801	0·4388	0·4743
February......	18 46 16	3·9063	9·5190	10·2894	1·8012	4·3890	4·7443	0·1801	0·4389	0·4744
March	18 49 13	3·9027	9·5080	10·2778	1·7995	4·3840	4·7389	0·1799	0·4384	0·4739
April	18 44 31	3·9014	9·5032	10·2728	1·7989	4·3818	4·7366	0·1799	0·4382	0·4737
May..........	18 48 26	3·9051	9 5104	10·2809	1·8006	4·3851	4·7403	0·1801	0·4385	0·4740
June	18 43 25	3·9012	9·5104	10·2794	1·7988	4·3851	4·7397	0·1799	0·4385	0·4740
July..........	18 46 1	3·9065	9·5043	10·2757	1·8012	4·3823	4·7379	0·1801	0·4382	0·4738
August	18 47 37	3·9068	9·5146	10·2856	1·8014	4·3870	4·7425	0·1801	0·4387	0·4742
September	18 43 52	3·9046	9·5065	10·2771	1·8004	4·3833	4·7386	0·1800	0·4383	0·4739

APPENDIX II.

Meteorological Observations.—Table I.

Kew Observatory.

Longitude $0^h\ 1^m\ 15^s{\cdot}1$ W. Latitude 51° 28′ 6″ N. Height above sea-level =34 feet.
Mean Monthly results from the continuous Records for the Twelve Months ending September 30th, 1882.

Months.	Thermometer.*					Barometer.†					Means of vapour-tension.
	Means.	Extreme maximum.		Extreme minimum.		Means.	Extreme maximum.		Extreme minimum.		
		Date.	Ther.	Date.	Ther.		Date.	Bar.	Date.	Bar.	
	°	d. h.	°	d. h.	°	inches.	d. h.	inches.	d. h.	inches.	inch.
1881. October	45·4	11 3 P.M.	61·7	17 7 A.M.	25·4	30·017	7 11 A.M.	30·516	14 7 A.M.	29·079	·244
November	49·0	5 noon	61·5	29 midnt.	29·3	29·962	13 10 P.M.	30·449	26 midnt.	28·889	·298
December	39·8	18 5 A.M.	53·5	23 ?	23·7‡	30·008	27 10 A.M.	30·655	18 4 A.M.	28·802	·219
1882. January	40·6	6 1 P.M.	52·8	25 9 A.M.	23·4	30·371	18 10 ,,	30·983	3 4 ,,	29·213	·223
February	42·5	26 noon	55·1	2 6 ,,	23·2	30·245	20 8 ,, 9 ,,	30·857	28 midnt.	28·924	·230
March	45·9	18 3 P.M. 4 ,,	61·7	23 5 ,,	30·6	30·021	16 8 ,,	30·661	1 4 A.M.	28·807	·242
April	48·1	20 3 ,, 21 1 ,,	62·7	16 5 ,,	33·6	29·785	8 8 ,,	30·367	29 5 P.M.	28·996	·250
May	54·3	22 2 ,,	71·9	17 5 ,,	36·8	30·055	17 8 ,,	30·504	25 noon	29·320	·295
June	56·7	27 5 ,,	72·2	17 5 ,,	41·3	29·916	1 1 ,,	30·351	10 2 A.M.	29·446	·344
July	59·9	3 4 ,,	75·5	1 ?	42·8‡	29·876	27 7 ,,	30·484	6 6 P.M.	29·321	·382
August	60·1	12 2 ,,	79·8	31 4 ,,	45·0	29·924	4 9 ,, 10 ,,	30 299	23 6 A.M.	29·236	·358
September	54·5	3 3 ,,	68·0	15 4 ,,	36·5§	29·868	8 10 ,,	30·396	27 6 ,,	29·229	·323
Means	49·7	....	..	....	..	30·004	....	..	....	..	·284

The above Table is extracted from the Quarterly Weather Report of the Meteorological Office, by permission of the Meteorological Council, except the temperatures and vapour-tensions for July, August, and September.

* The thermometers are 10 feet above the ground.
† Readings reduced to sea-level.
‡ Corrected reading of minimum thermometer, obtained from weather sheets.
§ Approximate reading.

Meteorological Observations.—Table II.

Kew Observatory.

Months.	Mean amount of cloud (0=clear, 10=over-cast).	Rainfall *.			Weather †. Number of days on which were registered						Wind ‡. Number of days on which it blew							
		Total.	Maxi-mum.	Date.	Rain.	Snow.	Hail.	Thun-der-storms.	Clear sky.	Over-cast sky.	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.
1881.		in.	in.															
October ..	6·4	2·395	0·900	22	17	..	1	..	7	13	7	6	7	1	1	4	3	2
November	7·4	2·495	0·455	24	21	..	..	..	1	15	..	..	4	2	6	17	1	..
December	6·9	2·505	0·640	17	20	4	..	..	3	17	4	2	..	1	7	12	4	1
1882.																		
January ..	8·1	1·260	0·415	8	14	..	..	..	1	22	..	1	1	3	6	14	5	1
February .	8·1	1·515	0·385	28	13	..	..	..	2	19	2	..	6	3	4	5	5	3
March ...	5·8	1·045	0·570	25	11	3	4	..	7	11	4	..	..	2	..	16	7	2
April	6·7	2·620	0·750	25	18	..	..	..	4	14	3	4	4	2	5	7	3	2
May	6·0	1·240	0·340	5	14	..	..	1	5	10	2	8	4	1	6	7	2	1
June	8·0	2·030	0·330	8 & 24	18	..	..	1	..	21	..	2	2	1	4	8	8	5
July	7·3	2·205	0·485	5	20	..	1	1	..	12	1	2	..	2	5	17	1	3
August ..	7·1	1·150	0·305	31	13	..	..	..	1	14	1	2	3	..	3	8	9	5
September	6·7	2·380	0·810	28	15	..	..	1	1	9	7	4	4	3	2	5	3	2
Totals..		22·840			194	7	6	4	32	177	31	31	35	21	49	120	51	27

* Measured daily at 10 A.M. by gauge 1·75 feet above surface of ground. † Derived from observations made at 10 A.M., noon, 2, 4, and 10 P.M.

‡ As registered by the anemograph.

Meteorological Observations.—Table III.

Kew Observatory.

Months.	Bright Sunshine.			Maximum temperature in sun's rays.			Minimum temperature on the ground.			Horizontal movement of the Air.*		
	Total number of hours.	Number of hours Sun was above the horizon.	Percentage of possible sunshine.	Mean.	Highest.	Date.	Mean.	Lowest.	Date.	Average daily Velocity.	Greatest Movement in a day.	Date.
1881.	h. m.	h. m.		deg.	deg.		deg.	deg.		miles.	miles.	
October	110 25	328 26	34	89·7	109·2	7	34·1	19·3	31	280	751	14
November	62 55	264 8	24	82·1	102·9	19	38·1	25·1	2	321	738	27
December	42 40	242 55	17	62·4	83·2	17	29·5	19·5	24	232	600	20
1882.												
January	31 35	259 7	12	61·3	84·7	9	31·8	20·4	25	218	559	2
February......	43 10	277 46	12	74·7	99·0	12	32·8	17·3	2	233	601	26
March........	155 20	366 47	42	101·4	120·0	30	33·6	20·7	4	279	594	26
April	165 35	414 33	40	110·5	121·8	20	34·9	26·0	11	337	616	29
May..........	258 30	482 4	54	122·2	134·2	29	38·8	27·9	17	260	549	20
June	142 0	494 22	29	120·1	133·3	20	44·7	33·7	16	271	543	5
July..........	202 50	497 00	41	127·1	139·0	21	49·2	40·3	1	256	463	7
August	147 10	419 31	33	120·4	138·7	2	47·5	37·3	9	221	494	23
September .. .	124 35	377 40	33	110·0	126·5	3	40·8	32·5	16	183	568	2

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* As indicated by a Robinson's anemograph, 70 feet above the general surface of the ground.

APPENDIX III.

List of Instruments, Apparatus, &c. the Property of the Kew Committee at the present date out of the custody of the Superintendent, on Loan.

To whom lent.	Articles.	When lent.
G. J. Symons, F.R.S.	Old Kew Thermometer Screen .... Portable Transit Instrument	1868 1869
The Science and Art Department, South Kensington.	The articles specified in the list in the Annual Report for 1876, with the exception of the Photo - Heliograph, Pendulum Apparatus, Kew Dip-Circle, Portable Unifilar, and Hodgkinson's Actinometer.	1876
Dr. T. Thorpe, F.R.S.	Three Open Scale Standard Thermometers, Nos. 561, 562, and 563.	1879
Major Herschel, R.E.	Invariable Pendulums, Nos. 1821, 4, and 11, Shelton Clock, R.S. No. 34. Stands, Telescopes, and Accessories.	1881
Mr. R. W. Munro ..	Standard Straight-edge....................	1881
Capt. Dawson, R.A. .	Unifilar Magnetometer by Jones, No. 102, complete, with three Magnets and Deflection Bar. Dip-Circle, by Barrow, one Pair of Needles, and Magnetizing Bars. Two Bifilar Magnetometers. One Balance Magnetometer. Two Declinometers. Two Tripod Stands.	1882
Rev. S. J. Perry, F.R.S.	Unifilar Magnetometer, No. 101, complete .... Dip-Circle by Barrow, No. 24, complete, with four Needles, and a Pair of Magnetizing Bars.	1882
Mr. Casella.........	Dip-Circle, by Barrow, with two Needles	1882
Dr. E. Ristori	Small Theodolite, by Robinson, No. C. 41 .... Old mahogany Declinometer, with Mirror Magnet, N.L. Tripod Stand.	1882
Major-General Sir H. Lefroy, R.A.	Two parcels Magnetical and Meteorological MSS. from the Sabine Magnetic Office.	1882

APPENDIX IV.

Working Receipts and Expenses of the Kew Observatory for the Ten Years 1872 to 1881 inclusive.

RECEIPTS.	1872.	1873	1874.	1875.	1876.	1877.	1878.	1879.	1880.	1881.
	£	£	£	£	£	£	£	£	£	£
British Association	240									
Gassiot Trust	480	608	499	500	499	498	496	496	497	497
Meteorological Committee	614	649	650	650	650	421	400	410	400	400
Verification and other Fees	104	349	270	456	419	456	585	470	539	596
Sales, &c.	8	2	63	36	15	24	5	15	18	17
Totals £	1,446	1,608	1,482	1,642	1,583	1,399	1,486	1,391	1,454	1,510
Number of Staff	9	9	11	13	12	11	11	13	13	11
PAYMENTS.	£	£	£	£	£	£	£	£	£	£
Salaries and Extra Work	935	863	1,006	1,063	1,101	924	959	1,094	1,092	1,071
House	152	108	143	162	173	133	140	148	150	151
Printing, Stationery, and Contingencies	117	140	129	110	158	134	160	170	159	145
Instruments, Apparatus, &c.	37	74	168	229	113	168	144	67	94	80
Experimental	...	4	60	22	38	15	12	13	95	3
Totals £	1,241	1,189	1,506	1,586	1,583	1,374	1,415	1,492	1,590	1,450
Cash Balances £	85	522	549	586	501	501	621	512	376	414

Extraordinary Payments met by Balances of previous Years.

	1872.	1873	1874.	1875.	1876.	1877.	1878.	1879.	1880.	1881.
	£	£	£	£	£	£	£	£	£	£
Salaries—Beckley, Mechanician	54	10	10	10	10					
Tuition	...	18	31	31	31					
Arrears of Work and Checking	...	...	29	...	49					
Honoraria and Gratuities	...	...	...	...	100	...	...	70	...	13
House—Furniture	...	...	...	28	8	12	...	...	...	10
Illness	...	...	...	...	...	...	...	...	...	20
Contingencies—Advertisements	...	...	...	...	...	...	...	17	...	5
Instrumental Repairs, &c. — Magnetographs	...	...	83							
Instrumental Repairs, &c. — Ther. Tester (Galton's)	...	...	...	64	...	4	...	...	35	
Instrumental Repairs, &c. — Cathetometers	...	...	...	...	...	71				
Sundries	...	...	...	...	...	16				
					Pendulums.	Stand. Bar.	Hyd. Press.		Sun-spots.	Cloud cam.
					16	14	45	...	90	13
Totals £	54	28	153	133	214	117	45	87	125	61

December 7, 1882.

THE PRESIDENT in the Chair.

The President announced that he had appointed as Vice-Presidents:—

The Treasurer.
Mr. J. Ball.
Professor Lister.
Professor Prestwich.
The Marquis of Salisbury.

Mr. Frederic Ducane Godman, Mr. Jonathan Hutchinson, and Mr. Walter Weldon were admitted into the Society.

It was announced that the question of the re-admission into the Society of Dr. H. E. Armstrong would be put to the vote at the next meeting.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Alterations of the Excitability of the Sensory Nerves of Man by the Passage of a Galvanic Current." By A. WALLER, M.D., and A. DE WATTEVILLE, M.A., B.Sc. Communicated by Dr. BURDON SANDERSON, F.R.S. Received October 26, 1882.

Hitherto the only experiments concerning this subject have been made on animals, the degree of sensory excitation being estimated by the amount of reflex action produced.* During our experiments on the alteration of excitability in the motor nerves of man we had the opportunity of noticing the occurrence of apparently similar phenomena in the sensory nerves of the skin.

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* Zurhelle, "De Nervorum Sensitivorum Irritibilitate in Statu Electrotoni," Berlin, 1864. Also "Untersuchungen aus d. Physiol. Labor. zu Bonn," 1865, p. 80. (Reference in Hermann, "Handbuch der Physiologie," 1879, vol. ii, part i, pages 46, 47.) Hällsten, "Elektrotonus i Sensibla Nerver," "Nordiskt Med. Arkiv." 1880, vol. xii, part v. (And in Du Bois Reymond's "Archiv," 1880, page 112. Reference in Virchow's "Jahresbericht," 1810, pages 202, 204, by Gad and Panum.)

This fact led us to undertake a preliminary series of experiments, of which we propose to give here the main results.

The methods we employed were precisely the same as those used for the investigation of the excitability of the motor nerves.* One electrode of small size—the exploring electrode—was fixed over the nerve chosen for the experiment; whilst the other electrode, of large size, rested on a distant part of the body. In order to secure the coincidence of the zones of polar alteration and of stimulation, the polarising and testing currents were united in the same circuit.

This is effected, when the induced current is used for testing the excitability, by including the secondary coil in the circuit of the polarising battery. When galvanic makes and breaks are so used, the current of the testing battery is thrown into the circuit of the polarising current without breaking the latter, by means of the double key of Helmholtz arranged in the usual manner.

The precautions we took for the elimination of errors arising from changes in the resistance of the body and in the current strengths during the experiments were either 1st, the galvanometric control of the currents used; 2nd, the intercalation of large additional resistances in the circuit (viz., 10,000 ohms, which is about 8 times the resistance of the human body under the conditions of our experiments). The influence of any changes in that resistance would thereby be diminished in the same proportion.

We usually employed the method of minimal stimuli, first noting the current strength required to produce, by its action on the normal nerve, a reaction in consciousness; then finding the changes of the current strength necessary to produce the same effect during and after polarisation, anodic and kathodic.

Two points of importance with reference to such experiments may briefly be alluded to here:—First, the necessity of carefully distinguishing between the continuous sensation produced by the polarising current when it has reached a certain intensity; second, the necessity of using a uniform rhythm of excitation, owing to the readiness with which stimuli summate in the sensory nerves of man. The sensations are referred either to the portion of the skin immediately under the electrode or to the parts supplied by peripheral distribution of the nerve. In experiments on mixed nerve-trunks care must be taken to eliminate the possible admixture of sensations due to muscular contractions.

Our general conclusion is, that during and after the passage of a galvanic current the alterations in the excitability of the sensory nerves of man follow a course essentially similar to those observed in the motor nerves.

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* "Proc. Roy. Soc.," vol. 33, p. 353; and "Phil. Trans.," 1882, p. 961.

Proof by Makes and Breaks of a Galvanic Current.

1. The effect of a make excitation is increased by the kathodic influence of the polarising current. This effect is, as usual, more marked in the polar zone (where the density is greater) than in the peripolar, and increases, within physiological limits, with the strength of the polarising current.

Placing the neutral electrode on the back and the exploring electrode over one of the cutaneous nerves at the wrist we obtained the following numbers. No external resistance was used, but the galvanometer showed that no change of resistance in the circuit took place during the experiment:—

Strength of polarising current............		0	2	4	6	8
Strength of current necessary to produce minimal sensation expressed in number of cells	In polar zone	9	6	5	4	2
	In peripolar zone	10	8	6	5	3

2. The effect of a break excitation in the polar zone is rapidly diminished and abolished by the anodic influence of the polarising current. (Owing to the strength of current required this can scarcely be ascertained for the peripolar zone.)

Arranging our electrodes as in the experiment first described, we take a current of thirty cells, which gives a distinct sensation at break. We then introduce a polarising current gradually increased. The sensation becomes rapidly fainter, and disappears altogether when the polarising current has reached five or six cells.

Proof by Induction Currents.

1. The effect of induction shocks is increased when the excitation falls upon the kathodic zone of the polarising current.

A series of experiments made with the testing electrode placed (according to the method above described) on various superficial nerves gave the following numbers: they are derived from experiments made with and without additional resistance (the numbers obtained having been reduced in the latter case). For the sake of comparison we also give the numbers obtained from experiments on motor nerves.

Strength of kathodic polarisation.	Variations of excitability of sensory nerves expressed in terms of distance of secondary coil.										Ditto, of motor nerves.	
	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	*a.*	*b.*
0................	100	107	110	122	127	110	127	100	104	115	116	113
Weak, 5—10 cells . .	142	115	118	128	133	120	133	115	148	130	127	118
Stronger, 10—20 cells	160	120	124	136	139	126	139	122	166	141	153	137

The number of cells given for the polarising current were those used when no resistance was added. In some of the experiments 10,000 ohms were thrown in, and a proportional increase in the number of cells became necessary (viz., about eight times as many as in the former instances):—

2. The effect of induction shocks is at first diminished, then increases to normal, or above normal, when the excitation falls upon the anodic zone of the polarising current. We suppose this effect, which is the same as that observed on the motor nerve, to be due to an invasion of the anelectrotonic by the katelectrotonic influence when the polarising current is increased beyond a certain strength. This invasion takes place more readily from the polar into the peripolar zone, than in the opposite sense.

Number of cells of polarising current.........		0	2	4	6	8	10	12	14	16	18	20	22	24	30	40
Distance of secondary coil necessary to produce minimal sensation..	Peri-polar	102	92	88	86	87	92	95	101	104	107	111	114	116		
	Polar ...	115	...	...	...	...	85	...	...	...	...	75	...	...	80	85

This experiment was made upon the cutaneous nerves of the back of the arm, the other plate resting on the leg. The galvanometer showed that the variations in the effect were unaccompanied by any changes in the resistances in circuit.

With reference to the after-effects of which the accurate determination offers considerable difficulties, we content ourselves with stating here that they appear to consist in an increase of excitability both after anodic and kathodic influence, preceded in the latter case by a short but appreciable diminution when the polarisation has been long and strong enough.

II. "Preliminary Notice of an Investigation into the Coagulation of the Perivisceral Fluid of the Sea-Urchin." By E. A. SCHÄFER, F.R.S. Received November 8, 1882.

The perivisceral fluid of the sea-urchin is a coagulable fluid of about the same specific gravity and chemical composition as sea-water. The corpuscles which float in it have been described by several observers, and recently very carefully by Mr. P. Geddes, in the "Archives de Zoologie Expérimentale" for 1880. The majority are pale and very amœboid, resembling the lymph-corpuscles of vertebrates, and the blood-corpuscles of most invertebrates. Others are more granular in appearance, and others again contain a reddish-brown colouring matter. The fluid in which they float is usually perfectly clear and colourless when the corpuscles are separated by filtration, and contains no appreciable amount of any proteid matter.

When the fluid is drawn from the shell into a glass vessel it rapidly undergoes what appears to be a sort of coagulation; the coagulum soon begins to shrink, and continues to do so to such an extent that at the end of a few hours it is reduced to but a small shred of coloured substance. In this respect the coagulum closely resembles that of vertebrate blood, and especially of frog's blood, which may also shrink in a few hours to a very small bulk.

If the clot is examined with the microscope it is found to contain all the corpuscles; and these are so closely arranged, and their processes are frequently so long and ramified, that it is difficult to make out the material in which they are embedded. The material of coagulation seems on this account to have been overlooked by Geddes, who refers the coagulum wholly to the remarkable massing together of the amœboid pale corpuscles, which he has observed and described.* It is easy, however, to demonstrate the presence of this material of coagulation, and to show that the phenomenon of coagulation is independent of the formation of masses or "plasmodia" of the corpuscles.

If the fluid be mixed as it flows from the shell with an equal volume of saturated solution of sulphate of magnesia, its coagulation is indefinitely delayed. With such a fluid the following experiments may be made:—

1. Diluted with water it immediately forms a coagulum which shrinks on standing as does the coagulum of the freshly drawn perivisceral fluid. The coagulum, when examined with the microscope, appears as a clear substance in which the cells, which are of course dead and for the most part rounded, lie separately embedded.

2. If the mixture be filtered all the corpuscles remain on the filter,

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* *Loc. cit.*, and "Proc. Roy. Soc.," vol. 30, p. 252.

and a clear fluid passes through. This gives no coagulum on dilution, but the corpuscles, if removed from the filter and suspended in a saturated solution of sulphate of magnesia, exude abundance of the coagulable material on the addition of water. This is not unlike the results obtained by Mr. Wooldridge in the Leipzig Laboratory with white corpuscles from the lymphatic glands and from the blood of mammals.*

I have spoken throughout of the substance, upon the formation or exudation of which from the corpuscles the clotting of the Echinus fluid seems to depend, as coagulable material, and not as fibrine. It does not, in fact, either in its chemical reactions or in its microscopic characters, bear any sort of resemblance to the fibrine of vertebrate blood, but appears to be more nearly allied to mucin, although the possession by it of the remarkable property of spontaneously shrinking after its first formation gives it a deceptive similarity to fibrine.

The detailed account of the above investigation will be published in the "Journal of Physiology."†

III. "Preliminary Note on the Structure, Development, and Affinities of Phoronis." By W. H. CALDWELL, B.A., Caius College, Cambridge, Demonstrator of Zoology. Communicated by Dr. M. FOSTER, Sec. R.S. Received November 24, 1882.

Owing to the time that must necessarily elapse during the preparation of plates, it has seemed to me advisable to publish the following preliminary account of my observations on the anatomy and development of Phoronis. These studies were made for the most part in the Zoological Station at Naples. I am much indebted to Dr. Anton Dohrn for his great kindness and assistance. I have not thought it necessary in this preliminary note to refer at any length to the observations of previous investigators,‡ and the bearing of the facts on recent morphological speculation has at most been indicated in the briefest possible manner. I would, however, specially refer to some observations on the development made in the summer of 1881, by Dr. Hatschek, who most generously not only sent me material, but on his return to Naples resigned his work and drawings to me.

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* "Proc. Roy. Soc.," vol. 32, p. 413; and "Archiv. f. (Anat. u.) Physiol.," 1881.

† The opportunity for carrying on these observations was afforded me at the Scottish Zoological Station of Professor J. Cossar Ewart and Mr. G. J. Romanes.

‡ J. Müller, Wagener, Krohn, Gegenbaur, Schneider, Kowalewsky, Metschnikow, Claparède, Wright, Dyster, Van Beneden, McIntosh, Wilson, &c.

Unfortunately, I have been unable to confirm Dr. Hatschek's account in several points. To do justice to this, I reserve a fuller account of the controverted points for my complete paper, when I hope also to reproduce Dr. Hatschek's figures of the living larvæ.*

STRUCTURE OF ADULT PHORONIS.

The mouth and anus are situated at one end. The short line between them is the median dorsal line. Between mouth and anus lies an epistome. *This epistome is the persistent præoral lobe* of the larva. New tentacles of the lophophore round the mouth are added on either side of the median dorsal line. There is a mesoblastic skeleton in the lophophore. The ventral surface is produced into a "foot," which constitutes the main part of the animal.

This determination of the surfaces depends on the development to be described below.

Epithelium of the Body.

Nervous processes of the ectoderm cells retain their connexion with the ectoderm, and concentrations, both of fibres and ganglion cells, occur in the skin outside the homogeneous basement membrane. The central nervous system remains therefore in the epidermis, *representing* the primitive condition.

Concentrations of the nervous system take place round the mouth to form a *postoral nerve-ring.* The anus lies outside this. The ring follows the line along the base of the tentacles, and has therefore like them the form of a horse-shoe. In front of this ring are situated a pair of sense organs, which I shall speak of as "ciliated pits." They lie in the concavity of the lophophore, on either side of the anus. They have the characteristic structure of sensory epithelium, consisting of sense cell, ganglion, and nerve-fibres. Sars† has figured in Rhabdopleura a pair of ciliated protuberances in what I hope to show is an homologous position.

A further concentration takes place in the form of a cord, which runs from the median dorsal part of the nerve-ring two-thirds of the length of the foot along its left side. It is therefore asymmetrical, and lies in the epidermis outside the basement membrane. Inside this nerve-cord lies an apparently hollow tube. This tube recalls the so-called large fibres of Chætopoda.

The alimentary canal is in the form of a ciliated tube, which may be divided into four main divisions, each characterised by a special epithelium :—

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* It is not to be understood that Dr. Hatschek agrees with my account of the facts of development.

† G. O. Sars, "On Rhabdopleura mirabilis," "Q. J. M. S.," vol. xiv, new series.

1. Œsophagus.
2. 1st stomach.
3. 2nd stomach.
4. Intestine.

The transition from the second to the third of these divisions is very marked. The third forms a small strongly ciliated chamber where the gut doubles on itself at the end of the foot.

The Body-Cavity

is lined throughout by peritoneum, which passes into mesenteries, dividing the cavity into several chambers. There is a ventral mesentery extending the whole length of the foot, attaching the outside of both descending and ascending limbs of the alimentary canal to the body-wall. Besides this there are two lateral mesenteries, which pass from the sides of the stomach to the body-wall. By these three mesenteries the *body-cavity is divided into three chambers*, viz., two anterior and one posterior. The lateral mesenteries end freely before the blind end of the foot is reached, so that all the chambers are here in full communication.

An important secondary connexion takes place some little way below the tentacles. The intestine attaches itself to the left lateral mesentery, dividing it into two parts, a shorter, attaching the intestine to the stomach, and a longer, attaching the intestine to the body-wall. Throughout the greater part of the foot this results in the posterior of the three chambers being divided into two.

The body-cavity is further divided by a septum, which passes from the line of the nerve-ring in the body-wall to the œsophagus, into two regions, viz. :—(i,) the space in front of the septum, *i.e.*, the body-cavity in the epistome and the tentacles ; (ii,) the space behind the septum, *i.e.*, the rest of the body-cavity.

Excretory System.

The genital pores discovered by Kowalewsky, by which he observed the ova to pass to the exterior, are the external openings of a pair of nephridia. Each nephridium consists of a simple ciliated tube, whose cell-walls are filled with brown concretions. *The tube opens into the posterior chamber of the body-cavity* on the sides of the lateral mesenteries. The external openings are situated in the regions subtended by the anterior divisions of the body-cavity.

Circulatory System.

A closed system of vessels, containing nucleated red corpuscles, is present.

The main vessels are two in number.

The afferent vessel to the tentacles divides at the median dorsal region of the septum.

Each half passes into a vessel lying at the base of the tentacles. From this cæcal vessels pass into these. A second vessel lying outside the former is also by means of a valvular arrangement in communication with the same cæcal vessels in the tentacles. From the outside ring on either side passes a lateral vessel to the ventral side of the œsophagus, where, joining its fellow of the opposite side in the left anterior division of the body-cavity, it runs as the single efferent vessel to the hind end of the foot, giving off numerous cæcal vessels in its course.

Further, there is a sinus round the stomach. This arrangement will be understood when its development is described below.

The walls of all the vessels are contractile.

Generative System.

The animals are hermaphrodite. The ova and spermatozoa are formed from cells of the efferent blood-vessel, which runs in the left anterior chamber of the body-cavity. Round this vessel lies the so-called "fat body," which is composed of large cells developed on the wall of the cæcal prolongation of the blood-vessel. The ova and testis lie in this tissue on opposite sides of the main vascular trunk. Thus the nerve-cord and the generative cells are asymmetrically placed. They lie on the left side of the foot.

DEVELOPMENT.

The following is a brief summary of the more important points:—

1. At the stage of four segmentation spheres, a division into two smaller clear and two larger opaque cells indicates the future ectoderm and endoderm.

2. The segmentation proceeds with considerable regularity, and results in a planula with half the cells smaller and less columnar than the other half.

3. Invagination of the larger cells almost obliterates the segmentation cavity, and a spherical gastrula with a blastopore is the final result of invagination. The gastrula becomes oval by the growth forwards of the ectoderm to form the præoral lobe, and the blastopore persists as the mouth.

The mesoblast is formed bilaterally from the endoderm on either side of the blastopore. From the time when two or three mesoblast cells are budded off on either side a cavity is present in each mass so formed. These cavities are the two halves of the body-cavity. I regard this mode of origin of the body-cavity as a modification by simplification of the enterocœl type, as described by Kowalewsky, in Argiope.*

Quite recently Metschnikow† has described the early stages of Phoronis. His account is very different to that given above. In the first place, he has not detected the origin of at least the main part of the mesoblast from the endoderm, as I have described. Further, Metschnikow has figured a blastula with four mesoblast cells in the segmentation cavity. Though I have made numerous complete series of sections through all stages of the blastula, I never have found any cells in the segmentation cavity. I would offer the following explanation of Metschnikow's account.

When the invagination to form the gastrula begins, the hypoblast cells previously cylindrical become very irregular, and project pseudopodia-like into the segmentation cavity. The free ends of these cells in actual sections are frequently cut off from their origin, and may then be mistaken for free cells lying in the segmentation cavity. They, however, never contain the nuclei of the cells. I would suggest that Metschnikow, who studied the development of Phoronis by means of optical sections of glycerine preparations of the whole larva, has mistaken these projecting ends of the amœboid endoderm cells for mesoderm cells.

Rapid growth of the mesoblast " diverticulum " into the præoral lobe takes place in such a way that distinct somatic and splanchnic layers, applied to the ectoderm and endoderm respectively, are easily to be recognised.

The cells soon become contractile, and the whole præoral lobe almost immediately after its appearance becomes actively so. The muscle cells have all the histological character of Mesenchyme, using this term in the sense used by the brothers Hertwig.‡ Meanwhile the ectoderm becomes thickened in two regions—

1. In the præoral lobe.
2. In the form of a postoral ring round the mouth.

The former becomes the future nervous ganglion ; the latter indicates the position of the line of future tentacles and the circumœsophageal nerve-ring of the older animal.

The anus is formed by a slight invagination of ectoderm behind the postoral ciliated ring on the opposite side of the body to that on which the mouth is placed, and is from the first terminal. The four divisions of the alimentary canal are now apparent, *i.e.*, the hypoblast cells have taken on their characteristic form in the several regions of the alimentary tract. The cells of the first stomach, however, *though ciliated*, are much more amœboid than in the adult. Throughout larval life *intra-cellular* digestion goes on in this region. This mode of digestion ceases with the metamorphosis.

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* A. Kowalewsky, "Protocol of the First Session of the United Sections of Anatomy, Physiology, and Comparative Anatomy at the Meeting of Russian Naturalists in Kasan, 1873 (Russian)."

† "Z. f. Wiss. Zool.," Ht. IV, 1882.

‡ "Die Cœlom-theorie," Jena, 1881.

With the formation of the anus this end of the body gradually grows out. The papilla with the anus at its end enlarges, and finally forms the largest part of the full-grown larva. Tentacles appear in pairs as outgrowths along the lines of the postoral ciliated ring, new tentacles appearing dorsally.

The further development of mesoblast proceeds always in continuity with the first pair of lateral diverticula. The body-cavity of the hind end of the larva is formed independently in a paired mass of cells which grows out from the end of the first formed sacs, and remains separated from the latter by a septum.

Thus the *whole mesoblast of the animal arises as two endodermic sacs*, the walls of which form somatic and splanchnic layers.

Nephridia.

On either side of the body lies a ciliated canal with cellular walls. This canal is *not* formed of perforated cells.

Each canal opens to the exterior behind the septum on either side of the opening of the foot. The canal lies *outside* the somatic mesoblast.

Attached to its inner blind end are a number of cells of very peculiar form. Each cell has a nucleus and processes similar to those of ordinary mesoblast cells. By one of these the cell is attached to the end of the large canal. This process is larger than the free processes, and has a cylindrical form. By the canal formed inside the cylinder, small brown concretions seen in the cell itself pass into the large canal, and so to the exterior. These excretory cells, with their fine canals, increase in number with the growth of the larva. They float freely in the body-cavity in front of the septum.

The cells are similar to the perforated cells which form the internal ends of the nephridia described by Hatschek in Echiurus.*

At no time during the free swimming life of larva does the excretory canal system open into the body-cavity.

With regard to the development of the nephridia I have observed somatic mesoblast cells, at the time when the mesoblastic sacs of the trunk are forming, take the characteristic shape of excretory cells with cylindrical processes.

On the other hand, I have failed to discover the origin of the main ciliated canal.

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* Berthold Hatschek, "Ueber Entwick. von Echiurus," Arbeit. a. d. Zool. Instit. Wien, vol. iii, 1880.

Dr. Hatschek believed that the whole organ was formed from the mesoblast cells mentioned above.

Vascular System.

The blood-vessels are all formed from the splanchnopleure.

The blood-corpuscles found in the vessels immediately after metamorphosis arise from mesoblast cells in front of the septum.

They form in masses which vary in number and position with the species, and lie free in the body-cavity held together by processes resulting from incomplete division. Each corpuscle has a nucleus, and with the growth of the larva the hæmoglobin colour gradually develops.

The vessels arise as splits in the splanchnopleure. The adult condition is reached partly by constrictions, partly by outgrowths from these. Thus we have at the close of larval life the blood-system in the following condition:—

1. Blood-corpuscles aggregated in two or more masses, lying free in the body-cavity of the præoral lobe, *i.e.*, in front of the septum.
2. A blood-vessel formed on the dorsal wall of the stomach, a marked structure in the larva.
3. The splanchnopleure sac, which in the region of the stomach forms a loose sac surrounding the gut.
4. Cæcal prolongations of this sac.
5. Cæcal prolongations into the rudiments of the adult tentacles.

Lophophore.

The larval tentacles are produced in pairs always towards the dorsal line, so that the most dorsal are the youngest. This is also the case in the adult Phoronis.

But the first rudiments of the adult set appear laterally, and new pairs are added both ventrally and dorsally to this pair, so that the oldest adult tentacles are not the most ventrally situated.

Full-formed Larva and Metamorphosis.

Finally we get the full grown free swimming larva, whose chief organs and their relative positions I shall briefly recapitulate.

The mouth and anus are at opposite ends of the ciliated body. The mouth is overhung by a large præoral lobe, whose margin is slightly thickened, and bears longer cilia than on the rest of the surface. This margin corresponds to the velum (præoral ring) of other larva.

I reserve the discussion of Kleinenberg's paper* on the origin of the nervous system, for my fuller paper. The bearing of the facts of Phoronis development on the question would involve matter of a too speculative character. Suffice it to say, that if the nerve-ring of Phoronis represents the nerve-ring of Cœlenterate ancestors, the præoral lobe must be regarded as a development of an anterior region of the sub-umbrella, while the anus has been formed in the region of the umbrella.

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* N. Kleinenberg, "Sull' Origine del Sistema Nervoso Centrale degli Annelidi." Reale Acad. d. Lincei, 1881.

The nervous elements of the ectoderm of the præoral lobe in all species, are concentrated into a ganglion (Scheitelplatte). In some species a large number of nerve fibres pass forwards from it to a sense organ. In one species four eye-spots are present. Behind the mouth an even number of tentacles form a postoral circlet. Behind these, and corresponding in number, lie rudiments of the adult tentacles. Along a line immediately in front of the larval circle, the ectoderm cells have become vacuolated. This change extends to a breadth of only three or four cells. Along a line at the base of the rudiments of the adult tentacles, the nervous prolongations of the ectoderm have formed a definite ring.

Round the anus a ring of very columnar ectoderm bearing strong cilia forms the chief organ of larval locomotion. The mouth opens into an œsophagus, which leads into a stomach. The stomach at its anterior end is produced into one or two ventral recesses.

In the vacuolated walls of these structures brown concretions are present.

The septum is attached in a circle along the line of the nerve-ring, and free communication exists between the body-cavity in front of the septum and the split in the splanchnopleure, which will form the blood sinus and vessels of the adult.

The condition of the rest of the vascular system we have already described in an old larva. The muscular arrangement in the invaginated foot is already similar to that in the adult.

The ventral mesentery still exists along the whole ventral surface from septum to anus. The pair of nephridia lie on either side of the body, their numerous excretory cells floating freely in the body-cavity in front of the septum. The external openings are placed one on either side of the opening of the foot.

The animal now swims to the bottom, and after swimming round and round many times on its own axis, and meanwhile undergoing violent contractions, suddenly begins to evaginate the foot. In fifteen to twenty minutes, a healthy individual will have become in all essential points like the adult.

During this time the following events take place :—

i. The whole præoral lobe with ganglion and sense organs pass into the stomach by the œsophagus. The rupture takes place along the line of vacuolated ectoderm mentioned above.

ii. The larval tentacles follow the præoral lobe.

iii. The blood-corpuscles pass inside the splanchnopleure sac by the opening described above, and break up in the sinus. From this they pass by contractions of the sac into the cæcal vessels and into the vessel which already exists in the dorsal side of the stomach.

iv. The larval excretory cells of the nephridia break off from the large canal and float freely into the body-cavity in front of the septum. They pass with the blood-corpuscles into the vessels. The *large canals remain as the paired nephridia of the adult.* The external openings by the changes undergone during the evagination have already almost their adult position.

v. The body-wall of the anal cone (at this stage) becomes folded, so as to present the appearance of columnar epithelium. By this process the invagination of the whole anal cone is rendered possible. The original anus is now half-way up the course of the ascending limit of the alimentary tract inside the foot, and the adult position of the nerve-ring is thus brought about.

The ventral mesentery extends along the outer curve of the alimentary tract along its whole extent, attaching the foot to the body-wall. At the end of the body it is seen in end view, apparently as a linear band, presenting a similar appearance to the funiculus of a polyzoon.

The ectoderm, from what has been said, must now re-attach itself by a secondary growth to the endoderm, along the lines where the præoral lobe broke off.

In the stomach the disintegrated cells of the tentacles and the præoral lobe with its ganglion and sense organs are now digested, not, however, by intercellular method, but, as in the adult, in the canal itself.

General Conclusions.

The life history of Phoronis, the chief points in which have been briefly mentioned, seems to offer a solution of many morphological problems.

These are of two kinds.

On the one hand we have those more general questions which concern the origin of various organs and systems of organs.

On the other those special problems which are concerned in solving the body plan of the different animal forms.

On the Origin of Organs and Organ Systems.

The condition of nervous concentrations in the ectoderm in Phoronis, both larval and adult, shows us how apparently new parts of the nervous system arise.

In Phoronis the præoral ring, corresponding to the velum of a

Trochosphære, is from the earliest stages reduced relatively to the postoral.

This latter, appearing in the gastrula stage, persists throughout life as a circumœsophageal ring. No anterior dorsal sensory part of the central nervous system exists in the adult.

The ganglion of the præoral lobe which in Chætopoda and Mollusca, &c., persists as the anterior sensory lobe of the brain, disappears with the change from a free to a fixed life.

The ganglion sense organs and velum of the præoral lobe are eaten during the metamorphosis.

The pair of sense organs are connected with the postoral nerve-ring.

In Capitellidæ, Dr. Eisig has been kind enough to inform me, the nerves from the ciliated pits are connected not with the anterior lobes of the brain, but with the posterior part from which the circumœsophageal commissures are given off.

Body-Cavity.

The whole body-cavity in the præoral lobe and in the trunk is an enterocœl. The closed vascular system is developed from the spanchnopleure. The intracellular excretory canals arise in somatic mesoblast cells. The existence of two divisions of the excretory system, viz.,

i. The intracellular closed canals;
ii. Large intercellular canals;

ceases with the metamorphosis. In Phoronis the atrophy of the intracellular system is coincident with the development of the vascular system.

If the intracellular excretory system of larval Phoronis is homologous with the similar excretory system in Platyelminthes, there is a presumption that the cavities in which the cells lie are homologous, that in fact Platyelminthes are degenerate enterocœles.

On the Relation of Phoronis to other Groups.

The most striking result of my researches on Phoronis is to give an explanation of the relation of Brachiopoda and Polyzoa to other animals. The identity of the Phoronis larva up to the formation of the nephridia, and before the outgrowth of the anal region, with the Trochosphære type of Hatschek is complete.

In Phoronis the body-cavity is an enterocœl. The distinction attempted to be drawn by the Hertwigs* between the histological characters of mesenchyme and mesoderm utterly breaks down in Phoronis.

I regard it, therefore, as probable that the other Trochosphæres are enterocœles.

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* O. and R. Hertwig, *loc. cit.*

The larvæ of Brachiopoda and Polyzoa I regard as modified from the Trochosphære by the earlier attainment of the relation of the ventral surface which in Phoronis is only accomplished during the metamorphosis.

Phoronis and Brachiopoda.

The conception of the body plan of Brachiopoda arising in this way involves an entirely new view of the homologies of the body surfaces. The following are the chief points which seem to me to determine these :—

1. The præoral lobe of the larva of Phoronis, and the so-called "segment" which bears the eye-spots in certain larval Brachiopoda* persist in part at least as the epistome of the adult.

2. There is a postoral nerve-ring in all the Brachiopoda I have examined) situated as in Phoronis in the ectoderm.

3. In both the body-cavity of the præoral lobe is separated from that of the rest of the body by a septum.

4. The "segments" of Brachiopoda are represented in Phoronis by the three divisions of the larva.

1. Præoral lobe as far back as the septum.
2. The rest of the body to the anal ring.
3. The invaginated foot.

An evagination similar to that in Phoronis of the third "segment" takes place in Brachiopoda when the larva fixes itself (Morse, Terebratulina.)

5. The tentacles arise from the line of the nerve-ring and are in the form of a horse-shoe, the outer curve of which is ventral. In the middle of the inner curve there is a break in the continuity of the tentacles. This interval is in the median dorsal line. On either side lies one of the youngest pair of tentacles.

6. The rectum when present lies in the posterior of the three main divisions of the body-cavity formed by the ventral and two lateral mesenteries (vide diagram B). The nephridia open to the interior in the posterior, to the exterior in the region of the anterior chambers of the body-cavity.

7. The same four divisions of the intestine are formed in both Phoronis and Brachiopoda.

Finally, I would point out that the so-called *segments of Brachiopoda are at right angles to the ordinary Chætopod segments.* This is easily seen to be the case by reference to the diagram, where AB represents the axis perpendicular to which ordinary segmentation takes place, CD that perpendicular to which Brachiopoda have been supposed to be segmented.

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* Kowalewsky, *loc. cit.*

Brachiopoda are thus fixed by their ventral surface.

The dorsal surface is indicated by the epistome, and the line between mouth and anus (when present).

Both valves of the shell are ventral.

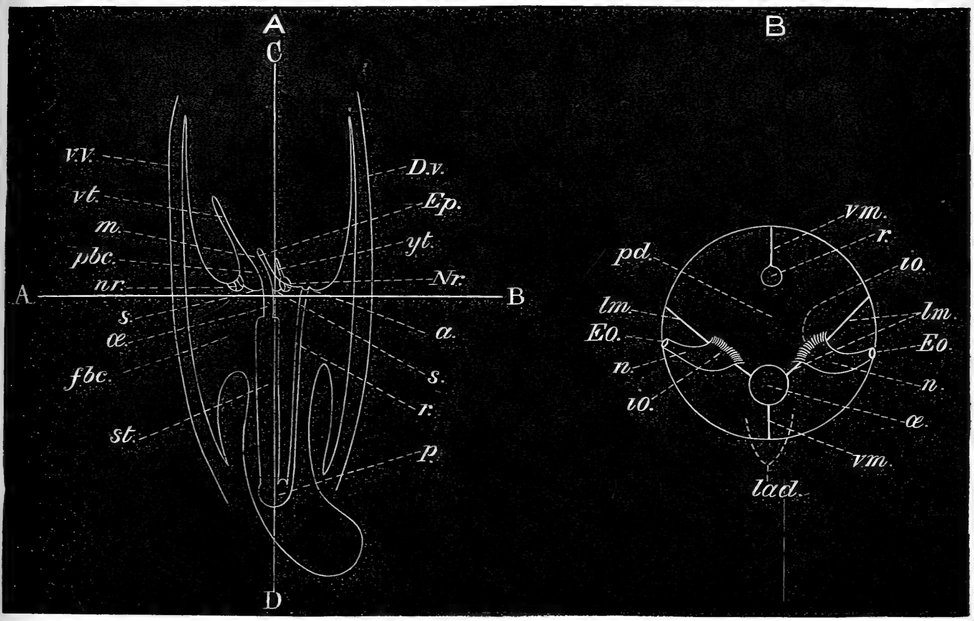
Phoronis and Polyzoa.

With regard to the Polyzoa, the evidence owing to the simplification which has taken place in their structure is not obtainable in the same quantity. I regard it, however, as probable that the epistome of Endoproct and Hippocrepian Polyzoa and the "foot" (Ray Lankester) in Rhabdopleura represent the præoral lobe.

The dorsal surface in Polyzoa is indicated as in Phoronis by the line between mouth and anus. If Phoronis, Brachiopoda, and Polyzoa have had segmented ancestors no trace of such remains in their ontogeny.

The discussion of the various views at present held on the Polyzoa and Brachiopoda must be deferred to my fuller paper.

So far as the facts of development and structure of Sipunculus and Phascolosoma are known, I see nothing to show that these forms are not referable to the same type of body structure as Phoronis, Brachiopoda, and Polyzoa. On the other hand, it seems quite possible that they may be further stages in degeneration from forms like Echiurus, which, after the researches of Hatschek, seem to be degenerated Chætopoda.



Two diagrams to illustrate the body plan of Phoronis, Brachiopoda, and Polyzoa.

A. Longitudinal vertical section through the adult animal.
B. Longitudinal horizontal section through the adult animal.
E.p. Præoral lobe or epistome.
m. Mouth.
a. Anus.
œ. Œsophagus.
s.t. First stomach.
p. Second stomach.
r. Intestine.
v.t. Right tentacle of ventral pair.
y.t. Right tentacle of dorsal pair.
p.b.c. Body-cavity of head.
f.b.c. Body-cavity of foot.
N.r. Postoral nerve-ring.
v.m. Ventral mesentery.
l.m. Lateral mesentery.
n. Nephridium.
E.o. External opening of nephridium.
i.o. Internal opening of nephridium.
p.d. Posterior chamber of body-cavity.
l.a.d. Lateral anterior divisions of body-cavity.
V.V. and D.V. Valves of Brachiopod shell.
A.B. Represents axis perpendicular to which ordinary segmentation takes place.
C.D. Represents axis perpendicular to which Brachiopoda have been supposed to be segmented.

December 14, 1882.

THE PRESIDENT in the Chair.

The Right Hon. Joseph Chamberlain, whose certificate had been suspended as required by the Statutes, was ballotted for and elected a Fellow of the Society.

In accordance with the announcement made from the Chair at the last Meeting, the question of Dr. Armstrong's re-admission was put to the vote, and was decided in the affirmative. The President thereupon declared that Dr. Armstrong was re-admitted into the Society.

The Presents received were laid on the table and thanks ordered for them.

The following Papers were read:—

I. "The Development of *Renilla*." By EDMUND B. WILSON, Ph.D. Communicated by Professor HUXLEY, F.R.S. Received October 5, 1882.

(Abstract.)

The following abstract contains the more important points established by a study of the development of *Renilla*, which was carried on at intervals during three years at the marine laboratory of the Johns Hopkins University, conducted by Dr. W. K. Brooks. The need of farther studies on the embryology of polyps in general must be apparent to every zoologist; and *Renilla*, as a highly specialised form, presents a number of special morphological problems, which can only be solved by a study of the embryological history of the organism. This genus appeared, therefore, to be a doubly desirable object for study.

The paper is divided into four parts. The first comprises an account of the segmentation of the egg and formation of the germ-layers; the second a description of the formation of the tissues and organs of the primary or axial polyp; the third part treats of the formation of the colony produced by budding from the axial polyp; and the fourth deals with a few theoretical questions suggested by the phenomena observed. The leading points of the paper are as follows:—

I.

(1.) As in other Alcyonaria, *Renilla* is diœcious, and fertilisation is effected in the water after discharge of the generative elements through the mouths of the feeding polyps. The ovarian or spermatic follicles are ruptured and cast aside at the time of spawning.

(2.) The action of the vitellus during segmentation is *extremely variable*, while the division of the nuclei appears to be nearly regular. The vitellus, after a preliminary division of the nuclei, may divide at the first cleavage into two, four, (?) eight, sixteen, or thirty-two spheres, which may be equal or noticeably unequal. In some cases cleavage begins at one pole a considerable time before any sign of activity is shown at the opposite pole, so that the segmentation appears precisely like that of a true meroblastic egg. In others, again, the segmentation is irregular and extremely unequal, so as to appear quite like that of an epibolic gastrula. All of these forms gave rise, however, to quite similar larvæ, which were proved by isolation in small aquaria to be normal and healthy.

In several cases of division into sixteen and thirty-two spheres, the vitellus was observed to undergo slight changes of form some time previous to actual division. These changes appear to be the expression of attempts at division on the part of the vitellus, which has not, however, energy enough to carry out a complete cleavage. In other cases the attempts are partially successful and the egg divides incompletely into spheres which do not become clear and well defined until the following cleavage. The series of forms appears to be produced by variations in the activity of the vitelline protoplasm or in the resistance (in the form probably of deutoplasm) which is opposed to it. The phenomena are of considerable interest, as showing how natural selection may find a field for action even in the earliest stages of an organism, and as a caution against drawing too hasty conclusions in regard to the character of segmentation from the study of a few individuals only. It is further remarkable to find the action of the vitellus and of the nuclei as independent of one another as these facts seem to indicate.

(3.) Cleavage is at first superficial, a considerable central mass remaining unsegmented. The furrows finally extend to the centre, and a small temporary segmentation cavity is formed.

(4.) The layers are separated by a process of delamination. The endodermic mass is at first solid and is not separated by a supporting lamella from the ectoderm.

(5.) The supporting lamella is derived mainly from the ectoderm by a very peculiar process of secretion at the inner ends of the cells.

(6.) The gastric cavity is formed by absorption of the central endoderm cells by those which are more peripherally placed. The former undergo a peculiar process of disintegration and form a granular *débris*, which is absorbed by the peripheral cells by a process which appears to be identical with the amœboid absorption of yolk observed by Reichenbach in the embryo crayfish. At this period the gastric cavity is without any communication with the exterior.

II.

(7.) The œsophagus is formed as a solid invagination of ectoderm, in which a slit-like cavity, elongated in the dorso-ventral plane, soon appears. The lower end of the œsophagus is then absorbed, placing the gastric cavity for the first time in communication with the exterior. Much variation exists in the process of absorption. Most commonly it begins at one side, so that the bottom of the œsophagus hangs down like a valve from the opposite side. It is then absorbed bodily.

(8.) The radial septa and the horizontal or peduncular septum differ widely in structure and mode of origin. The former arise simultaneously at the anterior end and grow backwards. Each septum consists of *two* layers of endoderm cells, separated by a structureless lamella. The peduncular septum arises at the posterior end and grows forwards. It is composed of *three* layers of endoderm cells, the middle of which atrophies. This septum is probably to be regarded as formed by the fusion of the dorsal pair of radial septa. The septa have a very marked bilateral arrangement.

(9.) The mesenterial filaments are formed as endodermic thickenings of the edges of the septa. After the formation of the mouth they become continuous with the invaginated ectoderm of the œsophagus. The filaments are arranged in pairs of different lengths and structure. The dorsal pair appears *last* and develops most slowly.

(10.) The tentacles appear simultaneously and are at first destitute of pinnæ. They arise as hollow cæcal outgrowths from the anterior extremities of the radial chambers.

(11.) The calyx-teeth are formed in a similar manner but in a definite sequence. The ventro-lateral pair first appear and then the median dorsal tooth. The remaining two pairs appear nearly at the same time, but the medio-lateral pair usually precedes the dorso-lateral. Occasionally, however, the reverse is true.

(12.) The muscles are entirely endodermic, with the possible exception of those of the tentacles. They are developed from the bases of epithelial cells, as "epithelio-muscular" cells or myoblasts. The cell-body, in many cases at least, becomes reduced to a small granular mass enclosing the nucleus and closely applied to the side of the fibre, and the entire muscle-element lies below the epithelium.

The muscles are arranged in two sets, longitudinal and circular, the latter being outside the former. The circular muscles form a nearly uniform sheet, but the longitudinal fibres are at first arranged in definite tracts which exhibit a striking bilateral symmetry.

(13.) The spicules are developed in the interior of cells and are of two kinds, ectodermic and endodermic, which differ widely in form and size. Professor B. K. Emerson has kindly examined them for me with the polariscope, and finds them to consist of a crystalline core, probably of aragonite, surrounded by an amorphous layer and this by a second crystalline layer, the axes of which correspond with those of the core.

(14.) The ventral chamber becomes closed in front by membranous outgrowths from the septa and body-walls. The dorsal chamber is closed by the forward extension of the fore edge of the peduncular septum, which finally unites with the dorsal wall of the body just anterior to the exhalent zooid (*vide infra*).

III.

(15.) The development of the buds is essentially like that of the axial polyp, but no trace of the peduncular septum is formed and the mesenterial filaments appear in a different sequence, the dorsal pair appearing first and developing most rapidly.

(16.) The buds which are to form sexual polyps appear always in symmetrically placed pairs, and, in the earlier stages, in a definite sequence. They are arranged in two simple lateral rows, which extend both forwards and backwards by the appearance of new buds upon the axial polyp. The backward extension is, however, limited, whence results the sinus, into which the peduncle is inserted. Anteriorly the two rows of buds extend forwards and downwards until they meet at the ventral side of the axial polyp, which is thus included within the disk.

New buds are constantly formed in the angles between older buds, and each lateral bud is in time enclosed by the younger adjacent buds in the same manner as the axial polyp.

(17.) The ventral sides of the buds are at first directed downwards, and hence, when the polyps in later stages bend upwards so as to assume a vertical direction, the ventral side is turned outwards, away from the centre of the disk.

(18.) The zooids develop in the same manner as the polyps and are indistinguishable from the latter in their early stages. They are at first single but soon multiply to form clusters, in which the ventral chambers of the zooids are always turned away from the centre of the group. The law of budding is therefore the same for the zooids and polyps. In rare cases the secondary zooids become in turn centres of multiplication according to the same law.

The axes of the primary zooids are definitely related to those of the polyps on which they are placed.

(19.) The "Hauptzooid" of Kölliker is formed at an early stage as a median bud upon the axial polyp, and its function is to discharge water from the colony. The other zooids draw in water from the exterior by the action of the cilia which line their cavities. This is true also of the sexual polyps in their early stages (though this function is entirely lost as they become older). Hence the zooids are physiologically, as well as anatomically, identical with the young polyps; they are, in other words, polyps in a state of arrested development.

The taking in of water is of vital importance to the organism, since the movements of the peduncle, by which the creature creeps, are effected by forcing the water to and fro. In this fact we find, probably, the explanation of the very early appearance of buds upon the axial polyp.

IV.

(20.) The facts of development, so far as they go, indicate the derivation of *Renilla* from a form related to the Bathyptileæ, which probably possessed a horny axis. This view is opposed to that of Kölliker, who considers that *Renilla* is related to the Penniformes only through a primitive simple "*Archiptilum*."

(21.) The following section contains a brief discussion of the bilateral symmetry, which is strongly exhibited, both in the individual polyps and in the entire colony. It is shown that in both cases the bilateral structure is correlated with a bilateral environment, which indicates a causal relation between the two; and I conclude that the bilateral environment determines the bilateral structure.

(22.) The last section contains a discussion of the polymorphism of *Renilla*. An attempt is made to show that the zooids are probably not degenerated polyps but buds in a state of arrested development, whose direct ancestors never possessed a more highly organised structure than at present.

Other theoretical questions suggested by the investigation are discussed under the various sections in the body of the paper.

II. "On the Morphology and the Development of the Perithecium of Meliola, a Genus of Tropical Epiphyllous Fungi." By H. Marshall Ward, B.A., Fellow of Owens College, Victoria University; late Cryptogamist to the Ceylon Government. Communicated by W. T. Thiselton Dyer, F.R.S. Received November 28, 1882.

(Abstract.)

The author has investigated the life-history, structure, and development of several species of these remarkable epiphytic fungi. The

much branched mycelium consists of jointed cylindrical hyphæ, with hardened brown or black cell-walls and finely granular protoplasmic contents; these are closely attached to the epidermis of tropical plants by rudimentary *haustoria*, which are closely adherent to the cuticle, but do not pierce the cells of the host.

The mycelium supports setaceous appendages of various forms, simple or branched; these *setæ* spring from scattered points along the course of the hyphæ, and are especially developed from around the fruit-bodies, arising from richly branched hyphæ in their neighbourhood. Bornet considered these as forming a special part of the fungus, under the name of the "receptacle," but development teaches that they cannot be regarded as of importance; no special function can be assigned to the *setæ*, and they are certainly not tubes for the outlet of the *spores*, as earlier observers surmised.

Other appendages occur as short, lateral, pyriform, or flasked-shaped branchlets; some of which become free, and develope new *mycelia* by budding, much as is the case with the *conidia* of *Erysiphe* and allied fungi. Others give rise to the *Perithecia*, which are globular or subglobular cases containing *asci* in their interior, and with hard black outer cell-walls.

The author has studied the origin and development of the *Perithecium* very particularly. A short ovoid or pyriform lateral branchlet becomes cut off as a unicellular body, by a firm septum close to the parent hypha; this cell is next divided into two by a cell-wall passing obliquely across it. Of the two cells thus produced, one divides more slowly, and forms a mass or "core" of thin-walled cells, with richly developed protoplasmic contents; the other, dividing much more rapidly, produces a layer of cells, which gradually envelopes the "core" of thin-walled cells, and forms the outer walls of the *Perithecium.*

The "core" of more slowly dividing thin-walled cells is an *ascogonium;* in later stages, certain of its constituents are seen to form the *asci* and *spores*, while others deliquesce and serve as nutritive material. The outer walls of the enveloping layers become thick, hard, and dark-coloured; the inner cells of the series deliquesce, and serve for nutrition of the young *asci*, &c., as before. All these processes are recognised in vertical sections of the young *Perithecium* in various stages, and are figured and described in detail.

The *asci* are delicate clavate sacs, developed successively, and containing two to eight uni-tri-septate *spores*, the formation of which is also figured. The germination of the *spores* was also examined; each puts forth a germinal tube, which soon developes a rudimentary *haustorium*, and becomes irregularly branched, finally growing forth as a *mycelium* like that first described.

The author examines and criticises the views held by Fries and

Bornet (who only worked with dried specimens), especially as to the systematic position of *Meliola*, and the opinion that they are representative tropical species of the European *Erysipheæ.* He shows that the original cell, from which the *Perithecium* is developed by continuous cell-multiplication, must be regarded as containing in itself the undifferentiated elements of an *Archecarpium* and *Antheridium-branch* (in the sense of De Bary and others); and that after the primary division into two unequal cells, we must look upon one of these—the more slowly divided *ascogenous* cell—as an *Archecarpium*, which produces the *asci* and *spores*, &c., while the other more rapidly developing cell may be considered the equivalent of the *Antheridium* and enveloping tissues of such a fruit-body as that of *Erysiphe.* Thus the sexual process, reduced to a minimum (physiologically) in *Erysipheæ*, has here disappeared entirely, the morphological equivalents of sexual organs being also further withdrawn. One step further, and we arrive at forms in which no trace of sexual organs exist. The *Meliolas* must therefore be regarded as a group, developed along similar lines to those of the *Erysipheæ*, *Eurotium*, &c., but in which the sexual process has become suppressed to a still greater extent.

With respect to the pathological action of these fungi on their hosts, the investigations show that no direct parasitic action of the *mycelium* is recognisable; the rudimentary *haustoria* do not injure the cell-contents, nor even pierce the cell-walls. Injury results indirectly, however, because the well developed *mycelium* deprives the leaves of light, air, &c., and blocks up the *stomata.*

III. "Note on a Discovery, as yet unpublished, by the late Professor F. M. Balfour, concerning the Existence of a Blastopore, and on the Origin of the Mesoblast in the Embryo of *Peripatus Capensis*." By Professor MOSELEY, F.R.S., and ADAM SEDGWICK, M.A., Fellow of Trinity College, Cambridge. Received December 4, 1882.

The late Professor F. M. Balfour left a considerable amount of material, both in the form of drawings and MSS., which he had intended to employ in the publication of a monograph on the anatomy and development of the members of the genus Peripatus, together with an account of all known species. The portions relating to anatomy and development have been prepared for the press, and will in the course of some months appear in the "Quarterly Journal of Microscopical Science" in full; but as some of the embryological results are of especial interest and of general morphological importance, it has been considered more expedient that these should at once be communicated to the Royal Society in the present note.

The Blastopore.—Balfour left no manuscript account or notes of his discovery in connexion with the drawings which he prepared in order to illustrate it, but he spoke about it to Professor Ray Lankester and also to us, and he further gave a short account of the matter in a private letter to Professor Kleinenberg, from which, by the courtesy of that distinguished embryologist, the following extract is made:—

"There is (in the early embryo of *Peripatus capensis*) along the whole ventral surface a groove which leads into the alimentary canal, and, as shown in sections, the walls of the alimentary canal give off pouches like those in Amphioxus, which form the mesoblastic somites. The hypoblast cells are large and filled with yolk, but the alimentary canal has a clear lumen. I have not yet got earlier stages, and the next later stages only differ from that described in the fact that the groove-like blastopore is closed and the mesoblastic somites are more numerous, while two widely separated thickenings of the epiblast constitute the first rudiments of the ventral nerve-cords."

The drawings left by Balfour in connexion with the above remarkable discoveries are four in number: one of the entire embryo, showing the slit-like blastopore and the pouch-like outgrowths of the archenteron, the other three depicting the transverse sections of the same embryo.

The first drawing, viz., that of the whole embryo, shows an embryo of an oval shape, possessing six somites, whilst along the middle of its ventral surface there are two slit-like openings, lying parallel to the long axis of the body, and placed one behind the other. The mesoblastic outgrowths are arranged bilaterally in pairs, six on either side of these slits. The following note in his handwriting is attached to this drawing:—

"Young larva of *Peripatus capensis.* I could not make out for certain which was the anterior end. Length 1·34 millims."

One of these openings is much longer than the other, and they present the appearance of having been once part of a single continuous slit, running nearly the whole length of the embryo. Balfour's own statement in the above letter, and two other embryos, which we have found among his material, prove that such is the case. These embryos, both taken from the uterus of the same female, are of two ages, but both are younger than that from which Balfour's drawing was made. The youngest is ·7 millim. in length and possesses three somites and a continuous slit extending along nearly the whole length of its ventral surface. The older one is ·86 millim. in length and possesses five somites. In this embryo the side walls of the middle portion of the slit have come together, preparatory to the fusion, which will almost immediately divide it into two parts. These two embryos have been drawn by Miss Balfour, and will be figured in the first part of the forthcoming memoir above referred to.

Origin of the Mesoblast.—Balfour's three remaining drawings are, as already stated, representations of transverse sections of the embryo figured by him as a whole. They show that, as he stated in the letter quoted, the mesoblast originates as a series of paired outgrowths from the hypoblast, and that these outgrowths are formed near the junction of the hypoblast with the epiblast at the lips of the blastopore. The mesoblast can be seen in the actual sections to have the form of paired sacs, the cells forming which pass continuously unto those of the hypoblast. One of them distinctly shows that at the stage with six somites, communications exist between the cavity of the mesenteron and that of the mesoblastic somites, and there is no need for us to enlarge upon the importance of these facts. Their close bearing upon some of the most important problems of morphology will be apparent to all, and we may, with advantage, quote here some passages from Balfour's "Comparative Embryology," which show that he himself long ago had anticipated and in a sense predicted their discovery.

"Although the mesoblastic groove of insects is not a gastrula, it is quite possible that it is the rudiment of a blastopore, the gastrula corresponding to which has now vanished from development." "Comparative Embryology," vol. 1, p. 378.

"TRACHEATA.—Insecta. It (the mesoblast) grows inwards from the lips of the germinal groove, which probably represents the remains of a blastopore." "Comparative Embryology," vol. 2, p. 291.

"It is, therefore, highly probable that the paired ingrowths of the mesoblast from the lips of the blastopore may have been, in the first instance, derived from a pair of archenteric diverticula." "Comparative Embryology," vol. 2, p. 294.

They were discovered in June last, only a short time before he started for Switzerland; we know but little of the new ideas which they called up in his mind. We can only point to passages in his published works which seem to indicate the direction which his speculations would have taken.

"In the first place it is to be noted that the above speculations render it probable that the type of nervous system from which that found in the adults of the Echinodermata, Platyelminthes, Chætopoda, Mollusca, &c., is derived, was a circumoral ring, like that of Medusæ, with which radially-arranged sense-organs may have been connected; . . . . Its anterior part may have given rise to supra-œsophageal ganglia and organs of vision; these being developed on the assumption of a bilaterally symmetrical form, and the consequent necessity arising for the sense-organs to be situated at the anterior end of the body. If this view is correct, the question presents itself as to how far the posterior part of the nervous system of the Bilateralia can be regarded as derived from the primitive radiate ring.

"A circumoral nerve-ring, if longitudinally extended, might give rise to a pair of nerve-cords united *in front and behind*,—exactly such a nervous system, in fact, as is present in many Nemertines (the Enopla and Pelagonemertes), in Peripatus and in primitive molluscan types (Chiton, Fissurella, &c.). From the lateral parts of this ring it would be easy to derive the ventral cord of the Chætopoda and Arthropoda. It is especially deserving of notice, in connexion with the nervous system of the above-mentioned Nemertines and Peripatus, that the commissure connecting the two nerve-cords behind is placed on the dorsal side of the intestines. As is at once obvious, by referring to the diagram (fig. 231 B), this is the position this commissure ought, undoubtedly, to occupy if derived from part of a nerve-ring which originally followed more or less closely the ciliated edge of the body of the supposed radiate ancestor." "Comparative Embryology," vol. 2, pp. 311, 312.

The facts of development here recorded give a strong additional support to this latter view, and seem to render possible a considerable extension of it along the same lines.

IV. "On the Refraction of Plane Polarised Light at the Surface of a Uniaxal Crystal. II." By R. T. GLAZEBROOK, M.A., F.R.S., Fellow and Lecturer of Trinity College, Demonstrator in the Cavendish Laboratory, Cambridge. Received December 4, 1882.

A paper of mine bearing the above title was read before the Royal Society in November, 1881, and has since been printed in the "Phil. Trans.," Part II, 1882. An abstract appeared in the "Proc. Roy. Soc.," No. 216, 1881.

While continuing with the same apparatus a series of measurements of similar nature, which have occupied me for the greater portion of the present year, I have just discovered a source of error which had hitherto escaped my notice, and which seriously affects all the results I have arrived at. I have been using a spectrometer made many years since by Grubb, of Dublin, for the late Dr. Robinson, of Armagh, and kindly lent to me by Professor Stokes.

A chance observation has showed me that the object-glasses of both collimator and telescope in this instrument are strongly doubly refracting.

If plane polarised light fall on either, the emergent beam is elliptically polarised. The defect is most marked with the object-glass of the collimator. If it be looked at between crossed Nicols in a pencil of parallel rays the field of view is bright, and is traversed by two brushes, hyperbolic in form, which for two positions of the lens become two straight lines, cutting each other at right angles.

If, again, plane polarised light be allowed to pass through the lens, and only the central part of the lens examined, while it is turned round its own axis I find that for four positions of the lens at right angles to each other the emergent beam is quenched by a Nicol placed with its principal plane at right angles to that of the polarising Nicol, the emergent light is plane polarised; but for all other positions of the lens the light is not quenched, but reduced to a minimum; the emergent beam is elliptically polarised, and the principal plane of the analysing Nicol is then (according to Fresnel's supposition) parallel to the minor axis of the elliptic vibration. Moreover, the position of this minor axis, as the lens is rotated, changes by about 25′.

Now, in the experiments described in my paper, it was supposed that plane polarised light, polarised in a known plane, fell on a certain prism of Iceland spar.

These recent experiments have shown me that the light was not plane polarised, and that even if we suppose the vibration along the minor axis of the ellipse to be so small that it may be neglected in our theoretical calculations, still the vibration along the major axis will differ considerably in direction from that of the plane polarised beam incident on the lens, while the angle between this major axis and the direction of vibration of the original beam will depend on the position of certain lines in the lens relatively to the plane of polarisation of the incident beam, and will vary as this position changes.

Moreover, the errors produced for two positions of this plane of polarisation differing by 90° will be the same in amount but opposite in sign, and this was the case in my experiments. It is certain that this defect must modify seriously the results of my experiments, it may possibly reduce to nothing the differences recorded in my paper between the electro-magnetic theory of light and experiment.

I have found, moreover, that the defect is a common one with lenses. Most of those I have tested since I observed it, some ten or twelve in number, show it to some extent. In none, however, is it so marked as in the one I have had the misfortune to use continuously during the past two and a half years in experiments on polarised light, all of which are affected by it. I am now endeavouring to procure a lens suitable for my purpose free from the defect, with the intention of repeating the experiments, and hope at some future time to lay my results before the Royal Society.

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Portrait of the Rev. Charles Turnor, Compiler of the "Collectanea Newtoniana." Also a Medal, formerly belonging to the same Rev. Charles Turnor. Mr. Anthony Hanmer.

December 21, 1882.

THE PRESIDENT in the Chair.

The Presents received were laid on the table and thanks ordered for them.

The following Papers were read :—

I. "On the Normal Paraffins. Part IV." By C. SCHORLEMMER, F.R.S., and T. E. THORPE, F.R.S. Received December 5, 1882.

(Abstract.)

This communication contains the results of an inquiry made to determine the constitution of the heptane which one of them found in the resinous exudation from *Pinus Sabiniana.* The pure heptane was converted into a mixture of chlorides, and these were transformed into the corresponding primary and secondary alcohols. The alcohol was then oxidised, and from the analysis of the silver salts it was proved that *Pinus* heptane had yielded primary heptyl alcohol and methyl-pentyl-carbinol, exactly as in the case of heptane from petroleum.

A second portion of the chlorides was converted into heptylene, boiling at 98·5°, which, unlike the hexylene of analogous constitution, combines only slowly with hydrochloric acid. The heptylene by oxidation was shown to be *butyl-methyl-ethylene*, $C_4H_9CH{=}CH.CH_3$; it had been formed from the secondary heptyl chloride—

$$C_4H_9—CH_2—CHCl.CH_3.$$

The results of the investigation afford further evidence of the fact that when chlorine acts upon a normal paraffin, not all the chlorides indicated by theory are formed, but only the primary chloride, and a secondary chloride, which contains the group—$CHCl.CH_3$.

II. "On the Connexion between the State of the Sun's Surface and the Horizontal Intensity of the Earth's Magnetism." By BALFOUR STEWART, F.R.S. Received December 6, 1882.

1. The late John Allan Broun, in a paper published in the "Transactions of the Royal Society of Edinburgh" (vol. xxii, Part 3), has compared together the daily changes of the earth's horizontal magnetic force at the four stations—Makerstoun (Scotland), Trevandrum (India), Singapore, and Hobarton, and has come to the following conclusions:—

(1.) The daily mean horizontal force increases at the same time at all the stations, and diminishes at the same time at all the stations.

(2.) The proportional amounts of increase and diminution at the stations are not very different from each other.

(3.) If this holds for all the points of the earth's surface, we may infer that the intensity of the magnetism of the whole earth is variable, increasing, or diminishing from day to day.

2. It is easy to see that the selection by Broun of the horizontal force component in preference to the vertical force or the declination was a happy choice. For since the magnetic system of the earth has two pairs of force-foci, one pair of which is, perhaps, chiefly affected by transient changes, the variations of declination at different stations might not possess that uniformity which those of horizontal force might be expected to exhibit; and in like manner the changes of vertical force might have opposite signs at opposite sides of the equator of the variable magnetic system.

On the other hand, at all stations in ordinary latitudes the horizontal force might be expected to increase or diminish at the same time, and nearly in the same proportion, whether the cause of this change were due to only one of the two magnetic systems, or whether it were shared by both.

3. Assuming, therefore, as the most probable conclusion that these changes of horizontal force represent changes in the intensity of the magnetism of the whole earth, let us now endeavour to ascertain whether they depend in any way on the state of the sun's surface.

In order to determine this point, I have taken the daily means given by Broun for the four stations above mentioned.

These are for the years 1844 and 1845, the unit being the same fraction of the horizontal force for each station. The next step has been to take the mean of these four daily means, and to assume that this represents approximately at least the value from day to day of the relative intensity of the magnetism of the earth.

Here it is necessary to remark that the indications recorded by Broun are those of a differential instrument (the Bifilar), which is not well adapted for recording long-period variations of the horizontal component in a trustworthy manner; but is, on the other hand, admirably suited for short-period variations. As it is with this latter species of change we have now to deal, we may therefore trust without hesitation to the instrumental records given by Broun.

4. The state of the sun's surface as regards spotted area for the years 1844 and 1845, will be found in an appendix to the Report of the Solar Physics Committee. The information there given is derived from Schwabe's sun pictures, and the whole amount of spotted area for each observation day is expressed in millionths of the sun's visible hemisphere.

5. The value of the comparison of these two records, terrestrial and solar, is lessened by the fact that in the four observatories Sunday was always a blank day, while bad weather caused the catalogue of Hofrath Schwabe to present many blank days, and even groups of days, during which it would be hazardous to estimate the spotted area by interpolation.

The comparison has been made in the following manner. The various days exhibiting a maximum of horizontal force have been taken as central epochs and compared with two days before and two days after, and this comparison has been extended to sun-spots, as well as to magnetic force. The method will be seen from the following specimen.

	Horizontal force.						Sun-spots area.				
Central date....	(1)	(2)	(3)	(4)	(5)	..	(1)	(2)	(3)	(4)	(5)
Jan. 4, 1844..	12·41	15·47	16·25	13·22	(13·48)	..	(81)	30	36	0	0
„ 16, „ ..	15·06	16·76	17·02	15·75	15·18	..	210	150	210	(202)	(195)
	..	..	..	..	..		..	..	..	..	..
April 24, 1844 .	18·53	18·11	19·68	14·77	9·88	..	480	420	516	396	390
„ 28, „ .	9·88	12·67	15·47	15·01	13·85	..	390	594	420	270	270

From what we have said, it will be inferred that the central dates represent the dates of (3), whether for horizontal force or sun-spots. The method is analogous to that adopted by Professor Loomis, when comparing together sun-spots and disturbances.

A comparison precisely similar was next made for days of magnetic minima.

6. The following results have been obtained from this comparison.

For magnetic maxima there were in all 59 cases in which the comparison was reasonably good, while for magnetic minima there were 49 such cases.

The mean of these is expressed as follows:—

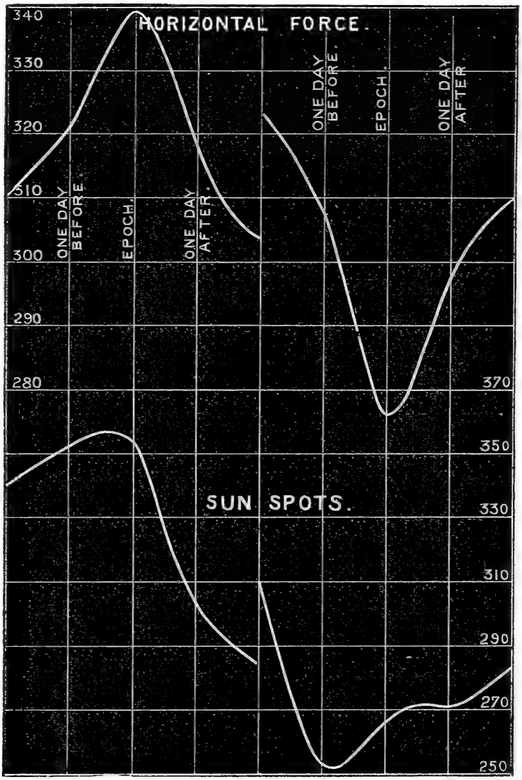
Table I.—Horizontal Force Maxima.

Horizontal force = a constant quantity *plus* the following expression, the unit being one hundred-thousandth of the whole horizontal force, increasing numbers denoting increasing force.						Spotted area in millionths of the sun's visible hemisphere.				
(1)	(2)	(3)	(4)	(5)	..	(1)	(2)	(3)	(4)	(5)
311 ·1	321 ·6	339 ·5	316 ·5	303 ·3	..	341	354	356	305	284

Table II.—Horizontal Force Minima (units as above.)

(1)	(2)	(3)	(4)	(5)		(1)	(2)	(3)	(4)	(5)
322 ·6	306 ·4	275 ·6	298 ·1	309 ·8	..	310	253	267	271	284

7. It will at once be seen from these results that high values of the horizontal force correspond to large sun-spot areas, and low values to small sun-spot areas. Thus the mean value of the horizontal force for the first or maximum series is 318·4, while for the second or minimum



series it is 302·5. Again, the mean sun-spot value for the first series is 328, while for the second series it is 277. A difference in mean spotted area of 51 millionths of the visible disk would therefore appear to correspond to a difference in terrestrial magnetic intensity equal to 15·9 hundred thousandths of the whole. The results of Tables I and II are exhibited graphically in the diagram.

8. If we refer to this diagram we shall see that the appearance of the curves representing magnetic change is very similar to that of the curves representing solar change, but that the epoch of maximum or minimum for the latter slightly precedes the corresponding epoch for the former.

The magnetic means recorded by Broun, from which these results have been derived, are for the Göttingen astronomical day (0 h. to 23 h.), while the sun-spot observations were made by Schwabe and Dessau, at times not far distant from noon (0 h.). Had the two sets of curves, terrestrial and solar, marched exactly together, we might thus have inferred that in reality the terrestrial (corresponding to 12 hours), was behind the solar (corresponding to times not far distant from 0 h.). But in addition to this, the curves denote a decided precedence of the solar over the terrestrial. There is thus considerable evidence in favour of a lagging behind on the part of the terrestrial results, and hence in this respect these magnetic phenomena of very short period form no exception to other magnetic phenomena, such as those connected with daily range, which exhibit a lagging behind the corresponding solar changes in a very unmistakable manner.

III. "On a Method of Photographing the Solar Corona without an Eclipse." By WILLIAM HUGGINS, D.C.L., LL.D., F.R.S. Received December 13, 1882.

Problems of the highest interest in the physics of our sun are connected, doubtless, with the varying forms which the coronal light is known to assume, but these would seem to admit of solution only on the condition of its being possible to study the corona continuously, and so to be able to confront its changes with the other variable phenomena which the sun presents. "Unless some means be found," says Professor C. A. Young, "for bringing out the structures round the sun which are hidden by the glare of our atmosphere, the progress of our knowledge must be very slow, for the corona is visible only about eight days in a century, in the aggregate, and then only over narrow stripes on the earth's surface, and but from one to five minutes at a time by any one observer."*

* "The Sun," p. 239.

The spectroscopic method of viewing the solar prominences fails, because a large part of the coronal light gives a continuous spectrum. The successful photograph of the spectrum of the corona taken in Egypt, with an instrument provided with a slit, under the superintendence of Professor Schuster during the solar eclipse of May 17, 1882, shows that the coronal light as a whole, that is the part which gives a continuous spectrum, as well as the other part of the light which may be resolved into bright lines, is very strong in the region of the spectrum extending from about G to H. It appeared to me, therefore, very probable that by making exclusive use of this portion of the spectrum it might be possible under certain conditions, about to be described, to photograph the corona without an eclipse.

In the years 1866–68 I tried screens of coloured glasses and other absorptive media, by which I was able to isolate certain portions of the spectrum, with the hope of seeing directly, without the use of the prism, the solar prominences.* I was unsuccessful, for the reason that I was not able by any glasses or other media to isolate so very restricted a portion of the spectrum as is represented by a bright line. This cause of unsuitableness of this method for the prominences, which give bright lines only, recommends it as very promising for the corona. If by screens of coloured glass or other absorptive media the region of the spectrum between G and H could be isolated, then the coronal light which is here very strong would have to contend only with a similar range of refrangibility of the light scattered from the terrestrial atmosphere. It appeared to me by no means improbable that under these conditions the corona would be able so far to hold its own against the atmospheric glare, that the parts of the sky immediately about the sun where the corona was present would be in a sensible degree brighter than the adjoining parts where the atmospheric light alone was present. It was obvious, however, that in our climate and low down on the earth's surface, even with the aid of suitable screens, the addition of the coronal light behind would be able to increase but in a very small degree the illumination of the sky at those places where it was present. There was also a serious drawback from the circumstance that although this region of the spectrum falls just within the range of vision, the sensitiveness of the eye for very small differences of illumination in this region near its limit of power is much less than in more favourable parts of the spectrum; at least such is the case with my own eyes. There was also another consideration of importance; the corona is an object of very complex form, and full of details depending on small differences of illumination, so that even if it could be glimpsed by the eye, it could scarcely be expected that observations of a sufficiently precise character could be made to permit of the detection of the more ordinary changes which are doubtlessly taking place in it.

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* "Monthly Notices," vol. xxviii, p. 88, and vol. xxix, p. 4.

These considerations induced me not to attempt eye-observations, but from the first to use photography, which possesses extreme sensitiveness in the discrimination of minute differences of illumination, and also the enormous advantage of furnishing a permanent record from an instantaneous exposure of the most complex forms. I have satisfied myself by some laboratory experiments that under suitable conditions of exposure and development a photographic plate can be made to record minute differences of illumination existing in different parts of a bright object, such as a sheet of drawing paper, which are so subtile as to be at the very limit of the power of recognition of a trained eye, and even, as it appeared to me, those which surpass that limit.

My first attempts at photographing the corona were made with photographic lenses, but uncertainty as to the state of correction of their chromatic aberration for this part of the spectrum, as well as some other probable sources of error which I wished to avoid, led me to make use of a reflecting telescope of the Newtonian form. The telescope is by Short, with speculum of 6 inches diameter, and about $3\frac{1}{2}$ feet focal length. A small photographic camera was fastened on the side of the telescope tube, and the image of the sun after reflection by the small plane speculum was brought to focus on the ground glass. The absorptive media were placed immediately in front of the sensitive film, as in that position they would produce the least optical disturbance. Before the end of the telescope was fixed a shutter of adjustable rapidity which reduced the aperture to 3 inches. This was connected with the telescope tube by a short tube of black velvet for the purpose of preventing vibrations from the moving shutter reaching the telescope. On account of the shortness of the exposures it was not necessary to give motion to the telescope.

It was now necessary to find an absorptive medium which would limit the light received by the plate to the portion of the spectrum from about G to H. There is a violet (pot) glass made, which practically does this. I had a number of pieces of this glass ground and polished on the surfaces. Three or four of these could be used together, castor-oil being placed between the pieces to diminish the reflection of light at their surfaces. Some inconvenience was found from small imperfections within the glass, and it would be desirable in any future experiments to have a larger supply of this glass, from which more perfect pieces might be selected.

In my later experiments I used a strong and newly made solution of potassic permanganate, in a glass cell with carefully polished sides. This may be considered as restricting the light to the desired range of wave-length, since light transmitted by this substance in the less refrangible parts of the spectrum does not affect the photographic plates.

Different times of exposure were given, from so short an exposure that the sun itself was rightly exposed, to much more prolonged exposures, in which not only the sun itself was photographically reversed, but also the part of the plates extending for a little distance from the sun's limb.

Gelatine plates were used, which were backed with a solution of asphaltum in benzole.

After some trials I satisfied myself that an appearance peculiarly coronal in its outline and character was to be seen in all the plates. I was, however, very desirous of trying some modifications of the method described with the hope of obtaining a photographic image of the corona of greater distinctness, in consequence of being in more marked contrast with the atmospheric illumination.

Our climate is very unpropitious for such observations, as very few intervals, even of short duration, occur in which the atmospheric glare immediately about the sun is not very great. Under these circumstances I think it is advisable to describe the results I have obtained without further delay.

The investigation was commenced at the end of May of this year, and the photographs were obtained between June and September 28th.

The plates which were successful are twenty in number. In all these the coronal form appears to be present. This appearance does not consist simply of increased photographic action immediately about the sun, but of distinct coronal forms and rays admitting in the best plates of measurement and drawing from them. This agreement in plates taken on different days with different absorptive media interposed, and with the sun in different parts of the field, together with other necessary precautions observed, makes it evident that we have not to do with any instrumental effect.

The plates taken with very short exposures show the inner corona only, but its outline can be distinctly traced when the plates are examined under suitable illumination. When the exposure was increased, the inner corona is lost in the outer corona, which shows the distinctly curved rays and rifts peculiar to it.

In the plates which were exposed for a longer time, not only the sun but the corona also is photographically reversed, and in these plates, having the appearance of a positive, the white reversed portion of the corona is more readily distinguished and followed in its irregularly sinuous outline than is the case in those plates where the sun only is reversed, and the corona appears, as in a negative, dark.

Professor Stokes was kind enough to allow me to send the originals to Cambridge for his examination, and I have his permission to give the following words from a letter I received from him: "The appearance is certainly very corona-like, and I am disposed to think it probable that it is really due to the corona." Professor Stokes's opinion was formed from the appearance on the plates alone, and without any knowledge of their orientation.

I have since been allowed, through the kindness of Captain Abney, to compare my plates with those taken of the corona in Egypt during the eclipse of May last. Though the corona is undergoing doubtless continual changes, there is reason to believe that the main features would not have suffered much alteration between May 17th and September 28th, when the last of my plates was taken. This comparison seems to leave no doubt that the object photographed on my plate is the corona. The more prominent features of the outer corona correspond in form and general orientation, and the inner corona, which is more uniform in height and definite in outline, is also very similar in my plates to its appearance in those taken during the eclipse.

Measures of the average height of the outer and of the inner corona in relation to the diameter of the sun's image are the same in the eclipse plates as they are in my plates taken here.

There remains little doubt that by the method described in this paper, under better conditions of climate, and especially at considerable elevations, the corona may be successfully photographed from day to day with a definiteness which would allow of the study of the changes which are doubtlessly always going on in it. By an adjustment of the times of exposure, the inner or the outer corona could be obtained as might be desired. It may be that by a somewhat greater restriction of the range of refrangibility of the light which is allowed to reach the plate, a still better result may be obtained.

Plates might be prepared sensitive to a limited range of light, but the rapid falling off of the coronal light about H would make it undesirable to endeavour to do without an absorptive screen. Lenses properly corrected might be employed, but my experience shows that excessive caution would have to be taken in respect of absolute cleanness of the surfaces and of some other points. There might be some advantage in intercepting the direct light of the sun itself by placing an opaque disk of the size of the sun's image upon the front surface of the absorptive screen. Though for the reasons I have already stated I did not attempt eye-observations, there seems no reason why with suitable screens and under suitable atmospheric conditions the corona should not be studied directly by the eye. There might be some advantages in supplementing the photographic records by direct eye-observations. I regret that the very few occasions on which it has been possible to observe the sun has put it out of my power to make further experiments in these and some other obvious directions.

Postscript.—Received December 15, 1882.

I have Captain Abney's permission to add the following letter this day received from him. "A careful examination of your series of sun-photographs, taken with absorbing media, convinces me that your claim to having secured photographs of the corona with an uneclipsed sun, is fully established. A comparison of your photographs with those obtained during the eclipse which took place in May last, shows not only that the general features are the same, but also that details, such as rifts and streamers, have the same position and form. If in your case the coronal appearances be due to instrumental causes, I take it that the eclipse photographs are equally untrustworthy, and that my lens and your reflector have the same optical defects. I think that evidence by means of photography of the existence of a corona at all is as clearly shown in the one case as in the other."—*Dec.* 15, 1882.]

IV. "On the Dark Plane which is formed over a Heated Wire in Dusty Air." By Lord RAYLEIGH, F.R.S., Professor of Experimental Physics in the University of Cambridge. Received December 8, 1882.

In the course of his examination of atmospheric dust as rendered evident by a convergent beam from the electric arc, Professor Tyndall noticed the formation of streams of dust-free air rising from the summits of moderately heated solid bodies.* "To study this effect a platinum wire was stretched across the beam, the two ends of the wire being connected with the two poles of a galvanic battery. To regulate the strength of the current a rheostat was placed in the circuit. Beginning with a feeble current, the temperature of the wire was gradually augmented; but before it reached the heat of ignition, a flat stream of air rose from it, which, when looked at edgeways, appeared darker and sharper than one of the blackest lines of Fraunhofer in the solar spectrum. Right and left of this dark vertical band the floating matter rose upwards, bounding definitely the non-luminous stream of air." . . . .

"When the wire is white hot, it sends up a band of intense darkness. This, I say, is due to the *destruction* of the floating matter. But even when its temperature does not exceed that of boiling water, the wire produces a dark ascending current. This, I say, is due to the *distribution* of the floating matter. Imagine the wire clasped by the mote-filled air. My idea is that it heats the air and lightens it, without in the same degree lightening the floating matter. The tendency, therefore, is to start a current of clean air through the mote-filled air. Figure the motion of the air all round the wire. Looking at its transverse section, we should see the air at the bottom of the wire bending round it right and left in two branch currents, ascending its sides, and turning to fill the partial vacuum created above the wire. Now as each new supply of air, filled with its motes, comes in contact with the hot wire, the clean air, as just stated, is first started through the inert motes. They are dragged after it, but there is a fringe of cleansed air in advance of the motes. The two purified fringes of the two branch currents unite above the wire, and, keeping the motes that once belonged to them right and left, they form by their union the dark band observed in the experiment. This process is incessant. Always the moment the mote-filled air touches the wire, the distribution is effected, a permanent dark band being thus produced. Could the air and the particles under the wire pass *through* its mass, we should have a vertical current of particles, but no dark band. For here, though the motes would be left behind at starting, they would hotly follow the ascending current, and thus abolish the darkness."

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* "Proc. Roy. Inst.," vol. 6, p. 3, 1870.

Professor Frankland,* on the other hand, considers that what is proved by the above described observations is that "a very large proportion of the suspended particles in the London atmosphere consists of water and other volatile liquid or solid matter."

Last summer (1881) I repeated and extended Tyndall's beautiful experiment, not feeling satisfied with the explanation of the dark plane given by the discoverer. Too much stress, it appeared to me, is placed upon the relative lightening of the air by heat. The original density is probably not more than about $\frac{1}{1000}$ part of that of the particles, and it is difficult to see how a slight further lightening could produce so much effect. In other respects, too, the explanation was not clear to me. At the same time I was not prepared to accept Professor Frankland's view that the foreign matter is volatilised.

The atmosphere of smoke was confined within a box (of about the size of a cigar-box), three of the vertical sides of which were composed of plates of glass. A beam of sunlight reflected into the darkened room from a heliostat was rendered convergent by a large lens of somewhat long focus, and made to pass in its concentrated condition through the box. The third glass side allowed the observer to see what was going on inside. It could be removed when desired so as to facilitate the introduction of smoke. The advantages of the box are twofold. With its aid much thicker smoke may be used than would be convenient in an open room, and it is more easy to avoid draughts which interfere greatly with the regularity of the phenomena to be observed. Smouldering brown paper was generally used to produce the smoke, but other substances, such as sulphur and phosphorus, have been tried. The experiment was not commenced until the smoke was completely formed, and had come nearly to rest. In some respects the most striking results were obtained from a copper blade, about $\frac{1}{4}$-inch broad, formed by hammering flat one end of a stout copper rod. The plane of the blade was horizontal, and its length was in the line of sight. The unhammered end of the rod projected from the box, and could be warmed with a spirit-lamp. The dark plane was well developed. At a moderate distance above the blade it is narrow, sometimes so narrow as almost to render necessary a magnifying glass; but below, where it attaches itself to the blade, it widens out to the full width, as shown in the figure. Whether the heated body be a thin blade or a cylindrical rod, the fluid passes round the obstacle according to the electrical law of flow, the stream-lines in the rear of the obstacle being of the same form as in front of it. This peculiarity of behaviour is due to the origin of the motion being at the obstacle itself, especially at its hinder surface. If a stream be formed by other means, and impinge upon the same obstacle without a difference of temperature, the motion is of a different character altogether, and eddies are formed in the shadow.

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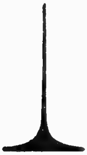
* "Proc. Roy. Soc.," vol. 25, p. 542.



The difference of temperature necessary to initiate these motions with this dark plane accompaniment is insignificant. On July 20, 1881, a glass rod, about $\frac{1}{4}$-inch in diameter, was employed. It was heated in a spirit-lamp, and then inserted in the smoke-box. The dark plane gradually became thinner as the rod cooled, but could be followed with a magnifier for a long time. While it was still quite distinct the experiment was stopped, and on opening the box the glass rod was found to be scarcely warmer than the fingers. It was almost impossible to believe that the smoky matter had been evaporated.

In order to test the matter more closely, smoke was slowly forced through a glass tube heated near the end pretty strongly by a spirit-lamp, and then allowed to emerge into the concentrated sunshine. No distinct attenuation of the smoke could be detected even under this treatment.

It is not necessary to dwell further upon these considerations, as the question may be regarded as settled by a decisive experiment tried a few days later. The glass rod before used was cooled in a mixture of salt and ice, and after wiping was placed in the box. In a short time a dark plane, extending *downwards* from the rod, clearly developed itself and persisted for a long while. This result not merely shows that the dark plane is not due to evaporation, but also excludes any explanation depending upon an augmentation in the difference of densities of fluid and foreign matter.

The experiment was varied by using a U-tube, through which cooled water could be made to flow. When the water was not very cold, the appearances were much the same as with the solid rod; but when, by means of salt and ice, the tube was cooled still further, a curious complication presented itself. Along the borders of the dark plane the smoke appeared considerably brighter than elsewhere. Sometimes when the flow was not very regular it looked at first as if the dark plane had been replaced by a bright one, but on closer examination the dark plane could be detected inside. There seems no doubt but that the effect is caused by condensation of moisture upon the smoke, due to the chilling which the damp air undergoes in passing close to the cold obstacle. Where the fog forms more light is scattered, hence the increased brightness. That the fog should not form within the smoke-free plane itself is what we might expect from the interesting observations of Aitken.

With respect to the cause of the formation of the dark plane, the most natural view would seem to be that the relatively dense particles are thrown outwards by centrifugal force as the mixture flows in curved lines round the obstacle. Even when the fluid is at rest, a gradual subsidence must take place under the action of gravity; but this effect could at first only manifest itself at the top where the upper boundary of the gas prevents the entrance of more dust from above. It is known that air in a closed space will gradually free itself from dust, but the observation of a thin dust-free stratum at the top of the vessel is difficult. If we conceive a vessel full of dusty air to be set into rapid rotation, the dust might be expected to pass outwards in all directions from the axis, along which a dust-free line would form itself. I have tried this experiment, but looking along the axis through the glass top of the vessel I could see no sign of a dark line, so long as the rotation was uniform. When, however, the vessel was stopped, a column of comparatively smoke-free air developed itself along the axis. This I attributed to the formation of an inward flow along the top of the vessel, combined with a downward flow along the axis after the manner described and explained by Professor James Thomson, so that the purified air had been in intimate proximity with the solid cover. It would almost seem as if this kind of contact was sufficient to purify the air without the aid of centrifugal force.

The experiments made hitherto in order to elucidate this question have given no decisive result. If the thin convex blade already spoken of be held in the smoke-box in a vertical instead of in a horizontal plane, the lines of motion are much less curved, and we might expect to eliminate the influence of centrifugal force. I have not succeeded in this way in getting rid of the dark plane; but since under the magnifier the curvature of the motion was still quite apparent, no absolute conclusion can be drawn.

V. "On the Origin of the Hydrocarbon Flame Spectrum." By G. D. Liveing, M.A., F.R.S., Professor of Chemistry, and J. Dewar, M.A., F.R.S., Jacksonian Professor, University of Cambridge. Received December 14, 1882.

In previous communications* to the Society we have described the spectra of what we believe to be three compound substances, viz., cyanogen, magnesium-hydrogen, and water.

In these investigations our chief aim has been to ascertain facts, and to avoid as far as possible adopting any special theory regarding the genesis of the spectra in question. Thus, in speaking of the magnesium-hydrogen spectrum, which consists of three sets of flutings closely resembling in character the flame spectrum of hydrocarbons, we remark:—"We have been careful to ascribe this line and its attendant series to a mixture of magnesium and hydrogen rather than to a chemical compound, because this expresses the facts, and we have not yet obtained any independent evidence of the existence of any chemical compound of those elements."

In dealing with the cyanogen spectrum, we sometimes refer to it as the "nitro-carbon spectrum," in order to convey that "we are dealing with a spectrum invariably associated with the presence of nitrogen and carbon, in such conditions that chemical union takes place." Finally, in summing up our observations on the spectrum of water, we remark:—"In writing of this and other spectra which we have traced to be due to compounds, we abstain from speculating upon the particular molecular condition or stage of combination or decomposition which may give rise to such spectra."

The difficulties we have met with in endeavouring exhaustively to clear up many apparently simple spectroscopic problems, on a basis of fact as opposed to theory, is further illustrated in the concluding remarks of our paper entitled "Investigations on the Spectrum of

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* "On the Spectra of the Compounds of Carbon with Hydrogen and Nitrogen." I and II. "Proc. Roy. Soc.," vol. 30, pp. 152, 494. "On the Spectrum of Carbon," *ib.*, vol. 33, p. 403. "General Observations on the Spectrum of Carbon and its Compounds," *ib.*, vol. 34, p. 123. "On the Spectrum of Water," *ib.*, vol. 30, p. 580, and vol. 33, p. 274. "Investigations on the Spectrum of Magnesium," *ib.*, vol. 32, p. 189.

Magnesium," wherein the following passage occurs:—"The chemical atoms of magnesium are either themselves capable of taking up a great variety of vibrations, or are capable by mutual action on each other, or on particles of matter of other kind, of giving rise to a great variety of vibrations of the luminiferous ether; and to trace satisfactorily the precise connexion between the occurrence of the various vibrations and the circumstances under which they occur, will require yet an extended series of observations." ("Proc. Roy. Soc.," vol. 32, p. 203.)

Specific spectra have been satisfactorily proved to emanate from the compound molecules of cyanogen, water, and magnesium-hydrogen, so far as we can interpret in the simplest way the many observations previously detailed. The fact that a fluted spectrum is produced under certain conditions, by a substance which does not give such a spectrum under other conditions, is of itself a proof that the body has either passed into an isomeric state or has formed some new compound; but we are not entitled to assert, without investigation, which of these two reasonable explanations of the phenomena is the true one. There is, however, a spectrum to which we have had occasion to refer in the papers on the spectra of the compounds of carbon, which closely resembles that of a compound substance, and which we, in common with some other spectroscopists, have been led to attribute to the hydrocarbon acetylene, without, however, being able to bring forward such rigid experimental proofs of its origin as we have adduced in the case of the three substances above referred to. In other words, the experimental evidence that the hydrocarbon flame spectrum is really due to a hydrocarbon was always indirect. Thus, we showed that many flames containing carbon, such as those of hydrogen mixed with bisulphide of carbon or carbonic oxide, and the flame of cyanogen in air, did not give this spectrum, and these particular flames are known, from the investigations of Berthelot, to be incapable of generating acetylene under conditions producing incomplete combustion. On the other hand, we found that a flame of hydrogen mixed with chloroform, which easily generates acetylene, gives the hydrocarbon flame spectrum in a very marked manner, and it is known that the ordinary blow-pipe flame, in which the same spectrum is well developed, contains this hydrocarbon.

These and other experiments point to the intimate relation of hydrogen and carbon in the combined form of acetylene to the production of this spectrum during combustion. In our various observations on the spectrum of the electric arc taken in different gases, the flame spectrum was always noticed, and seemed to be independent of the surrounding atmosphere. In the mode in which those experiments were conducted, it was easily shown that the carbons were never free from hydrogen, and that the gases always contained traces of aqueous vapour. Under these conditions acetylene is formed synthetically during the electric discharge, the line spectrum of hydrogen being absent; so that we were never convinced that the spectrum was not due to the former substance.

It is well to remark in passing, that our previous work on the spectrum of the carbon compounds was mainly directed to that particular spectrum which is characteristic of the flame of cyanogen, and only indirectly to the flame spectrum of hydrocarbon. We were further supported in connecting the latter spectrum with acetylene, by observing that cyanogen compounds are continuously formed when the arc discharge takes place in gases containing nitrogen, and that in all probability their formation is due, as Berthelot has shown, to a reaction taking place between acetylene and nitrogen. Berthelot is positive in his assertions that cyanogen is never formed by a direct combination between carbon and nitrogen, and that any such apparent combination is due to impure carbon, or to the presence of an imperfectly dried gas; in other words, hydrogen is essential to the production of cyanogen under such conditions according to the views of Berthelot. Such considerations led us to suggest the following view, expressed at the time the experiments were made, as to the origin of the hydrocarbon flame spectrum.

"The mere presence of the latter spectrum feebly developed in the electric discharge in compounds of carbon supposed to contain no hydrogen, appears to us to weigh very little against the series of observations which connect this spectrum directly with hydrocarbons." ("Proc. Roy. Soc.," vol. 30, p. 160.)

"The arc in the middle of a magnesia crucible often shows no trace of the hydrocarbon set, although the cyanogen are strong. If, however, puffs of air or carbonic acid are passed into the arc, the hydrocarbon lines are produced."

"When the hydrocarbon spectrum is strong the brilliancy and number of the cyanogen groups that are visible are undoubtedly increased, so that the one variety of vibrations seems to affect the other. This is easily accounted for by the chemical interaction which takes place between acetylene, nitrogen, and hydrocyanic acid. The hydrocarbon spectrum is brought out at once in the magnesia crucibles by moistening one of the poles. All such actions seem to show that hydrogen is essentially connected with the production of this fluted spectrum just as nitrogen is with the cyanogen series." (*Ib.*, vol. 34, pp. 126, 127.)

The fact that carbonic oxide, which is one of the most stable binary compounds of carbon, forms a distinct spectrum of a character similar to that of the flame spectrum, tended to support the view that the flame spectrum might originate with acetylene. The similarity in the character of the magnesium-hydrogen spectrum to that of the hydrocarbon flame spectrum induced us to believe that they were due to similarly constituted compounds, and inasmuch as we felt sure about the accuracy of the view which assigns the former spectrum to some compound of magnesium with hydrogen, we accepted the analogy in favour of the supposition that acetylene is the substance which produces the flame spectrum; or, at any rate, that acetylene is a necessary concomitant of the reaction taking place during its emission, and consequently might give rise to this peculiar spectrum.

Having examined this question in the way described, we adopted the view* of Angström and Thalén as to the genesis of this spectrum in opposition to the views of Attfield, Morren, Watts, Lockyer, and others, who held that this spectrum was really due to the vapour of carbon. The delicate character of the experiments which were required to discover the origin of the peculiar set of flutings in the more refrangible part of the spectrum of cyanogen made it apparent that, whatever views as to the origin of the hydrocarbon flame spectrum were adopted by different workers, experimental proof was still wanting to show which was the correct one. In referring to the theory that carbon vapour is the cause of the peculiar spectrum of cyanogen, we remarked: "Now, the evidence that carbon uncombined can take the state of vapour at the temperature of the electric arc is at present very imperfect. Carbon shows at such temperatures only incipient fusion, if so much as that, and that carbon uncombined should be vaporised at the far lower temperature of the flame of cyanogen is so incredible an hypothesis that it ought not to be accepted if the phenomena admit of any other probable explanation." ("Proc. Roy. Soc.," vol. 30, p. 506.)

With the object of being able to exhaust this question, a special study was subsequently made of the ultra-violet line spectrum of carbon, in order to ascertain whether any of its lines could be found in the spectra of the arc or flame. We have found that the ultra-violet lines of metallic substances have as a rule the greatest emissive power, and are often present when no trace of characteristic lines in the visible part of the spectrum can be detected. If carbon resembled the metals in this respect, then we might hope to find ultra-violet lines belonging to its vapour, thus enabling us to detect the volatilisation of the substance at the relatively low temperatures of the arc and flame. The test experiments made on this hypothesis are recorded in the paper entitled "General Observations on the Spectrum of Carbon and its Compounds." It is there shown that some seven of the marked ultra-violet spark lines of carbon occur in the spectrum of the arc discharge, although one of the strongest lines, situated in the visible portion of the spectrum at wave-length 4266, could not be found. Further, it is proved that the strongest ultra-violet line of carbon does occur in the spectrum of the flame of cyanogen fed with oxygen. Thus it seems probable that the same kind of carbon molecule exists, at least in part, in the arc and flame, as is found to be produced by the most powerful electric sparks, taken between carbon poles or in carbon compounds.

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* This view was first suggested by Plücker and Hittorf in the same paper in which they published the theory of spectra of different orders. They write:—"It appears doubtful that the different types depend solely upon temperature. If so, the temperature varying in the different parts of the ignited vapour of carbon, different types may be seen simultaneously. We shall not now discuss the influence which the coexistence of foreign gases might have on the spectra of the vapour of carbon, nor may we here decide whether or not in the lower temperature of the flame, a gaseous compound of carbon, not being entirely decomposed, exhibits with the spectra of the vapour of carbon simultaneously the spectrum of the undecomposed gas."—"Phil. Trans., 1865." (Jan. 26, 1883.)

Now the spark gives us the spectrum which is associated with the highest temperatures, and therefore it is assumed that this spectrum is that of the simplest kind of carbon vapour. If that be the case, we cannot avoid inferring that denser forms of carbon vapour may exist in arc and flame, emitting, like other complex bodies, a fluted, in contrast to a line, spectrum; or rather that the two distinct kinds of spectra may be superposed. Such considerations showed that a series of new experiments and observations must be made with the special object of reaching a definite conclusion regarding the origin of the flame spectrum, and the following paper contains a summary of the results of such an inquiry.

Vacuous Tubes.

We have heretofore laid little stress on observations of the spark in vacuous tubes on account of the great uncertainty as to the residual gases which may be left in them. The film of air and moisture adherent to the glass, the gases occluded in the electrodes, and minute quantities of hydrocarbons of high boiling-point introduced in sealing the glass, may easily form a sensible percentage of the residue in the exhausted tube, however pure the gas with which it was originally filled. The excessive difficulty of removing the last traces of moisture we learnt when making observations on the water spectrum, and the almost invariable presence of hydrogen in vacuous tubes is doubtless due in great measure to this cause. Wesendonck ("Proc. Roy. Soc.," vol. 32, p. 380) has fully confirmed our observations as to this difficulty. By a method similar to that employed by him, we have, however, succeeded in so far drying tubes and the gases introduced into them that the hydrogen lines are not visible in the electric discharge. For this purpose the (Plücker) tube was sealed on one side to a tube filled for some six or eight inches of its length with phosphoric anhydride through which the gas to be observed was passed, and on the other side to a similar tube full of phosphoric anhydride, which was in turn connected by fusion to the (Sprengel) pump. To dry the gas it is not enough to pass it through such a tube or even a much longer one full of phosphoric anhydride; it has to be left in contact with the anhydride for several hours, and to get the adhering film of moisture out of the tube it has to be heated after exhaustion, while connected as above described with the drying tubes, up to the point at which the glass begins to soften, and kept at near this temperature for some time. To get most of the gases out of the electrodes the tube must be exhausted and sparks passed through it for some time before it is finally filled with the gas to be observed. Even when these precautions have been taken, the lines of hydrogen can often be detected in tubes filled with gases which should contain no hydrogen. The general result of our observations on the spectra observed in tubes so prepared is that the channelled spectrum of the flame of hydrocarbons is not necessarily connected with the presence of hydrogen;* it does not come and go according as hydrogen is or is not present along with carbon in the way that the channelled spectrum of cyanogen comes and goes according as nitrogen is present or absent. Our observations confirm those of Wesendonck on this point.

A tube filled with hydrogen containing a small percentage of cyanogen and exhausted, was found to give plainly the seven channellings in the blue and six channellings in the indigo characteristic of cyanogen, and the hydrogen lines of course strongly, but no more than a trace of the brightest green line of the spectrum of the flame of hydrocarbons. The use of a Leyden jar brought out no more. Continued sparking made no sensible difference, the cyanogen spectrum remained, the green line did not alter: and no other line of the spectrum of the hydrocarbon flame appeared.

Tubes filled with carbonic oxide exhibit in general at different stages of exhaustion the following phenomena. When the exhaustion is commencing and the spark will just pass, the spectrum of the discharge in the capillary tube is usually that of the flame of hydrocarbons and nothing else. As the exhaustion proceeds the spectrum of carbonic oxide makes its appearance superposed on the former, and gradually increases in brilliance until it overpowers and at last, at a somewhat high degree of exhaustion, entirely supersedes the flame spectrum. This is when no jar is used. In the earlier stages of exhaustion the effect of the jar is to increase the relative brilliance of the flame spectrum and diminish that of the carbonic oxide spectrum, and at the same time to bring out strongly the lines of oxygen and carbon; at a certain stage of the exhaustion, when the flame spectrum is very weak without the jar, the effect of the jar is to bring it out again but without sensibly enfeebling the carbonic oxide spectrum, and without bringing out the carbon lines. At a still higher stage of exhaustion, when the carbonic oxide spectrum alone is seen without the jar, the flame spectrum is sometimes, not always, brought out by putting on the jar, though the carbon lines again show well. At this stage, at which the flame spectrum is not seen at all, the distance between the striæ in the wide part of the tube is considerable, and much metal is thrown off the electrodes, which are rapidly heated by the discharge. Some of these tubes were filled with carbonic oxide made by heating a mixture of potassium oxalate and lime contained in a prolongation of the tube containing the phosphoric anhydride. Others were filled from a mixture of sodium oxalate and sulphuric acid heated in a flask sealed to a long tube which had the middle part filled with quicklime and the two ends filled with phosphoric anhydride; and after all the air was expelled and the flask had been sealed off, the quicklime was heated to absorb the carbonic acid. These tubes showed no trace of the cyanogen flutings at any stage of exhaustion, either to the eye or in photographs of the spectrum. Nevertheless, in the earlier stages of exhaustion, some of such tubes do show, when the jar is used, a group of three lines in the indigo which is seen in the flame of cyanogen, and has formerly been described by us as part of the spectrum of cyanogen. We must now, however, retract the opinion that this group is due to cyanogen. We have before noted ("Proc. Roy. Soc.," vol. 34, pp. 125, 127) that these three lines are seen under many different circumstances when the cyanogen flutings are absent, and as the flutings also are frequently seen without the three lines, it seems that the three lines belong to some other spectrum than the flutings, and as we have now found them where nitrogen has been carefully excluded, we are forced to attribute them either to carbon or some compound other than cyanogen.

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* This statement may have to be qualified if the spectrum described as the second spectrum of hydrogen by Plücker and Hittorf and by Wüllner, and recently further investigated by Hasselberg ("Mem. Imp. Acad. Sc., St. Petersburg," xxx, No. 7), be the most persistent spectrum of hydrogen at low pressures; because the statement in the text is based on the supposition that hydrogen could be detected with certainty by the "C" or "F" line. (Jan. 26, 1883.)

In one case a very little copper-nitride was introduced into one end of the drying tube, and after the whole had been filled with gas and the generating flask sealed off, the nitride was heated so as to mix a small quantity of nitrogen with the carbonic oxide. In the spectrum of this tube the cyanogen flutings were not visible to the eye, but the ultra-violet set between K and L came out plainly in the photographs. No hydrogen line could be detected in it. This is remarkable, because Berthelot did not find that cyanogen is generated by electric sparks in a mixture of nitrogen and carbonic oxide unless hydrogen be also present. It would, however, be rash to assume that no trace of hydrogen was present because the lines of hydrogen were not visible in the spectrum, since we know by experience that the electric discharge does not always light up all that is in a tube. Mercury, for instance, must be present in all these tubes, but its lines do not usually show until the exhaustion is carried to a high degree.

One tube was filled with nitrogen with which was mixed a small percentage of cyanogen obtained by heating a little mercury-cyanide placed at one end of the drying tube. This tube gave well the channellings in the blue and indigo characteristic of cyanogen, but neither the hydrogen lines nor the spectrum of the hydrocarbon flame. The cyanogen seems to be scarcely at all decomposed by the spark when thus diluted, for the carbon lines were not brought out by the use of a jar, and the cyanogen spectrum remained after continued sparking. We remark that Berthelot found it exceedingly difficult to get acetylene from a mixture of cyanogen and hydrogen at atmospheric pressure; and in order to make this experiment succeed, short and very powerful sparks had to be continued for some hours.

In a tube filled with carbonic oxide mixed with a little air imperfectly dried, when not too highly exhausted, the carbonic oxide spectrum, that of the flame of hydrocarbons, and that of cyanogen, may all be seen at once superposed when no jar is used. With a jar and a tolerably high exhaustion the carbonic oxide spectrum, the hydrocarbon flame spectrum, and the carbon line spectrum, may all be seen at the same time. All the foregoing observations were made when the tubes were viewed end on and the image of the narrow part of the tube thrown on the slit by a lens. Tubes filled with carbon disulphide and carbon tetrachloride at reduced pressures have been examined by us, but these compounds of carbon are very quickly decomposed by the spark, so that few observations can be made with one tube. If the exhaustion is not carried very far the spectrum of the hydrocarbon flame is seen both in carbon disulphide and in carbon tetrachloride, when all the precautions above mentioned have been taken to remove moisture, and when no trace of the hydrogen spectrum is visible. At higher exhaustions the spectrum is a faint continuous one together with that of sulphur or chlorine as the one or other is present. The spark taken without condenser between electrodes near together in wide tubes filled with saturated vapour of carbon disulphide or carbon tetrachloride dried with phosphoric anhydride and deprived as completely as possible of air by pumping or boiling out, shows the spectrum of the flame of hydrocarbons brightly; but in tubes filled with carbonic acid gas from ignited sodium carbonate and boric anhydride and sealed off at a high temperature, the spectrum is that of carbonic oxide together with that of oxygen.

Spectrum of the Spark in Compounds of Carbon at Higher Pressures.

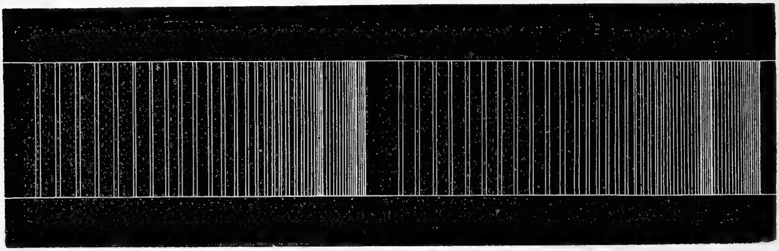
In the spark taken between poles of purified graphite in hydrogen, the spectrum of hydrocarbon flames is seen, and it increases in brilliance as the pressure of the gas is increased up to ten atmospheres, and continues bright at still higher pressures so far as we have observed, that is, up to twenty atmospheres. The spark without condenser in carbonic oxide at atmospheric pressure, shows both the spectrum of carbonic oxide and that of the hydrocarbon flame; and as the pressure of the gas is increased, the former spectrum grows fainter, while the latter grows brighter, no jar being used. The line spectrum of carbon is also visible. At the higher pressures the flame spectrum predominates and is very strong. The observations were carried up to a pressure of twenty-two and a half atmospheres. On letting down the pressure, the same phenomena occur in the reverse order. All the parts of the flame spectrum, as seen in a Bunsen burner, are increased in intensity as the pressure is increased. The fact that the effects of high pressure are so similar to those produced by the use of a condenser at lower pressures, seems to point to high temperature as the cause of those effects. But against this, we have the fact that at reduced pressure we get in carbonic oxide, the carbonic oxide spectrum and the line spectra of carbon and oxygen simultaneously, without that of the hydrocarbon flame. As we cannot doubt that a very high temperature is required to give the line spectrum of carbon, we must suppose that reduced pressure is unfavourable to the stability of the molecular combination, whatever it be, which gives the hydrocarbon flame spectrum. Wesendonck has remarked (*loc. cit.*) that in carbonic acid at pressures too low for the flame spectrum to be developed without a jar, it is only in the narrow part of the tube that the use of a jar brings out that spectrum. It would appear, therefore, that the constraint, due to the confined space in which the discharge occurs, has the same effect, in regard to the stability of the combination producing the spectrum in question, as increase of pressure.

Cyanogen Flame Spectrum.

Our former observations "On the Flame Spectrum of Cyanogen Burning in Air" were made on cyanogen gas, prepared from well-dried mercury cyanide, which was passed over phosphoric anhydride, and burnt from a platinum jet fused into the end of the tube. We observed what Plücker and Hittorf had noted, that the hydrocarbon bands were almost entirely absent, only the brightest green band was seen, and that faintly. When gaseous cyanogen is liquefied by the direct pressure of the gas, the researches of Gore ("Proc. Roy. Soc.," vol. 20, p. 68) have shown that it is apt to be contaminated with a brownish, treacley liquid, which probably arises from the imperfectly purified or dried cyanide of mercury. In order to obtain pure cyanogen we have prepared quantities of liquid cyanogen, not by compression, but by passing the already cooled gas into tubes placed in a carbonic acid and ether bath. By this method of condensation any easily liquefiable substances are isolated, and any permanently gaseous substance escapes. The samples were sealed up in glass tubes into which different reagents were inserted. After such treatment the cyanogen was used for the production of the flame in dry air or oxygen. The liquid cyanogen was left in contact with phosphoric anhydride, Nordhausen sulphuric acid, and ordinary sulphuric acid. By means of a special arrangement of glass tubing surrounding the flame dry oxygen could be supplied, or oxygen made directly from fused chlorate of potash could, by means of a separate nozzle, be directed on to the flame, and thus perfectly dry and pure gases used for combustion. Liquid cyanogen which had remained in presence of the above reagents gave only the single green hydrocarbon line faintly in dry air, all the cyanogen violet sets being strong. When oxygen made directly from the chlorate of potash was directed on to the flame, all the hydrocarbon flame sets appeared with marked brilliancy. The set of lines which we have formerly referred to as the three lines or set of three flutings of the cyanogen spectrum, show marked alteration of brilliancy with variations in the oxygen supply. Thus liquid cyanogen, purified by the action of the above reagents, does yield the spectrum of hydrocarbons on combustion in pure oxygen. From the great precautions we have taken we feel sure that the amount of combined hydrogen in the form of water or other impurities in the combining substances must have been exceedingly small, and that the marked increase in the intensity of the flame spectrum when oxygen replaces air is essentially connected with the higher temperature of the flame, and is not directly related to the amount of hydrogen present. This being the case, it must be admitted that the hydrocarbon flame spectrum requires a higher temperature for its production during the combustion of cyanogen than that which is sufficient to cause a powerful emission of the special spectrum of the molecules of cyanogen. Now the two compounds of carbon which give the highest temperature on combustion are cyanogen and acetylene. Both of these compounds decompose with evolution of heat, in fact they are explosive compounds, and the latent energy in the respective bodies is so great that if kinetic in the separated constituents it would raise the temperature between three and four thousand degrees. The flames of cyanogen and acetylene are peculiar in respect that the temperature of individual decomposing molecules is not dependent entirely on the temperature generated by the combustion which is a function of the tension of dissociation of the oxidised products, carbonic acid and water. We have no means of defining with any accuracy the temperature which the particles of such a flame may reach. We know, however, that the mean temperature of the flames of carbonic oxide and hydrogen lies between two and three thousand degrees, and if to this be added that which can be reached independently by the mere decomposition of cyanogen or acetylene, then we may safely infer that the temperature of individual molecules of carbon, nitrogen, and hydrogen in the respective flames of cyanogen and acetylene may reach a temperature of from six to seven thousand degrees.

A previous estimate of the temperature of the positive pole in the electric arc made by one of us, gave something like the same value.

Further evidence of the high temperature of the cyanogen flame is afforded by the occurrence in the spectrum of that flame, when fed with oxygen, of a series of flutings in the ultra-violet, which appear to be due to nitrogen. The series consists of four, or perhaps more, sets, each set consisting of a double series of lines overlapping one another. The lines increase in their distance apart on the more refrangible side, otherwise the flutings have a general resemblance to the B group of the solar spectrum. The accompanying figure gives the general appearance of two of the sets, but is not drawn to a scale.



The four sets commence approximately at about the wave-lengths 2718, 2588, 2479, 2373 respectively. They are frequently present in the spectrum of the arc taken in a magnesia crucible, and show strongly in that of the spark taken without a condenser either in air or nitrogen. As they appear in the spectrum of the spark in nitrogen, whether the electrodes be aluminium or magnesium, and do not appear when the spark is taken in hydrogen or in carbonic acid gas, they are in all probability due to nitrogen. When a large condenser is used they disappear.

The formation of acetylene in ordinary combustion seems to be the agent through which a very high local temperature is produced, and this is confirmed by the observations of Gouy on the occurrence of lines of the metals in the green cone of the Bunsen burner, which are generally only visible in spark spectra; on this view acetylene is a necessary agent in the production of the flame spectrum during combustion. The fact that when the arc is taken in a magnesia crucible, although the cyanogen spectrum is strong, the flame spectrum is often invisible, but may be made to appear by introducing a cool gas or moisture, may be accounted for by an increased resistance in the arc producing temporarily a higher mean temperature. Experiments in course of execution, where the arc will be subject to a sudden increase of pressure, will, we trust, solve this problem.

Electric Discharge between Graphite Poles in different Gases.

When pure graphite is employed, instead of the ordinary carbon poles, and the arc discharge is taken in different gases, in the same way as was described in our first paper, "On the Spectra of the Compounds of Carbon," we have noticed some slight differences which are worthy of record. In carbonic acid gas, fine channellings are seen covering the whole extent of the spectrum from the low red as far as the blue set of the flame spectrum, the flutings of the one group being observable as far as the next group. The triple set of the cyanogen flame spectrum remained very strong when all the cyanogen groups in the violet had disappeared. When the carbonic acid is displaced by hydrogen, the hydrogen lines appear, the hydrocarbon flame and the triple sets remaining bright; but in this gas the flame group at 431 is particularly well marked, and the carbon line at 4266 keeps flashing in occasionally. Thus we have in the same field of view the hydrocarbon series, the hydrogen lines, and one of the strongest lines of carbon. The De Meritens intermittent arc discharge was employed in these experiments, and it is curious to note that hydrogen, instead of favouring the passage of the arc between carbon poles, really introduces some peculiar resistance, perhaps owing to the reduction of temperature by the gas surrounding the arc, or because of the formation of acetylene. The arc is at any rate much shorter and smaller in section than in air, but the temperature seems to be correspondingly increased, as we may infer from the fact that the hydrogen and carbon lines are now very marked. The Siemens arc in air does not show the carbon line at 4266, although we have proved that some of the chief ultra-violet lines occur in this discharge. The arc taken in carbonic oxide shows the triple group along with the usual sets of the hydrocarbon flame spectrum, without any trace of the carbonic oxide spectrum being visible. The occurrence of the triple set of lines, in the absence of other groups characteristic of cyanogen, makes us doubt whether this set has anything really to do with nitrogen. We are inclined to think that their previous appearance when the arc was taken in glycerine containing nitro-benzole was really due to some indirect effect, and ought not to be taken as proof of the formation of cyanogen in the absence of other characteristic groups under such circumstances.

VI. "On the Inversion of the Blastodermic Layers in the Rat and Mouse." By ALEXANDER FRASER, M.B., &c., the Owens College, Manchester. Communicated by ALLEN THOMSON, F.R.S. Received December 18, 1882.

Having been engaged since May, 1881, in the investigation of the early placentation in some of the Rodentia, rat, mouse, and guinea-pig, in which it presents peculiar features, I was so fortunate in the commencement of July of the present year to ascertain the fact that in the common grey rat and its white variety, an arrangement of the blastodermic layers existed, similar to that which had been known in the guinea-pig since the publication of Bischoff's observations in 1852.

I was able, early in August, to extend this fact so as to include the common house mouse and its white variety.

These facts were ascertained by the examination of entire series of microscopic sections made in three different planes of the uterine loculi of rats and mice pregnant from the eighth to the tenth day. For the method used in imbedding I am indebted to the kindness of Professor His, Director of the Anatomische Anstalt in Leipzig.

These facts were communicated to Dr. Thomson early in July, and formed the subject of an oral statement made to the Biological Section of the British Association at the Southampton Meeting in August.

At that period my observations did not cover a stage early enough to enable me to give a satisfactory explanation of the inversion, but since that date I have been occupied in tracing the history of the ova in the rat, mouse, and guinea-pig from the time of their leaving the ovary up to the thirteenth day of pregnancy. My observations on the ova up to the sixth day are as yet incomplete, but as the inversion can be explained from ova of this date and onwards, I do not hesitate to offer them to the Society.

From the sixth day onwards to the thirteenth I have made, at intervals of four hours, series of sections from pregnant loculi in three different planes as already stated.

The remarks which are to follow will have reference chiefly to the rat (so far as the ovum is concerned) in which animal my work has been more complete than in the other two, and I shall deal first with the changes taking place in limited areas of the mucous membrane of the uterus, leading to the formation of the decidua, and onwards to the fully formed maternal portion of the placenta, and secondly with a brief outline of the developmental phenomena, so far as these are peculiar, from the sixth day onwards to the thirteenth.

The Decidua.

So far as the history of this structure is concerned, the following description will answer equally well for any one of the three animals under consideration.

It is formed by the transformation of the tissue lying between the epithelium of the uterus and the muscular wall. This change is accompanied by the total disappearance of the uterine glands, and consists in the formation of a large mass of round-celled material, occupying the whole circumference of the uterine tube in each loculus. The centre of this decidual mass is occupied by a prolongation of the uterine cavity formed by the extension of the decidual substance round it, while the main cavity of the uterus remains for a time continuous throughout the whole length of the uterine tube. This continuity is interrupted about the ninth day by the obliteration of the cavity caused by the increased decidual growth, with the exception of that part of the cavity in the centre of the decidual mass in which the ovum is situated.

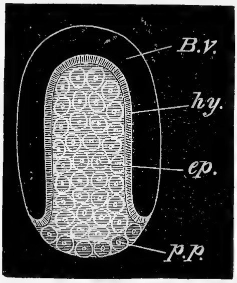
In the early stage the ovum is equally surrounded by decidua on all sides, but as development proceeds the decidua at the free side of the uterus gradually diminishes and disappears. Before this takes place a separation between the decidua and the muscular wall of the uterus occurs in this region, but not until about the sixteenth day, and the slight connexion which then exists between the decidua and the free side of the uterus can be recognised as a white line passing transversely over the pregnant loculus on removing it from the abdominal cavity. In this manner the uterine cavity becomes again continuous throughout the whole length of the uterine tube. The changes taking place in the decidua at the mesometrial side of the uterus depend upon the formation of the maternal portion of the placenta; in this region the decidua becomes vacuolated, these vacuoles forming the maternal vessels, lined at first by a single layer of flat cells, and which with the blood-vessels of the fœtal portion of the placenta soon come into intimate relation.

This decidua, present in the three animals under consideration, differs remarkably in its history and extent from that of other Mammals in which the development of the blastodermic layers is regular, and it would appear that there is a close association between the form and extent of this decidua and the peculiar modification of the ordinary type of development which I am now about to describe.

The Ovum.

Of the many pregnant loculi of which I have made sections between the sixth and seventh day, the one which affords the key to the explanation of this deviation from the ordinary type of development is from a rat pregnant six days and twelve hours. This ovum has been cut parallel to its long axis, which lies in the transverse vertical axis of the uterus. (Fig. 1.)

FIG. 1.



Section of ovum from rat, 6 days and 12 hours.

The ovum measures in its long axis 0·16 millim., in breadth 0·06 millim.

Of the zona there is no trace. The ovum has reached the stage of a blastodermic vesicle, *B.v.*, the wall of which is formed over the greater part of its circumference by a delicate membrane, in which there lie sparsely scattered cells. At the placental pole of the ovum, *p.p.*, or at that part of the circumference turned to the mesometrial side of the uterus, there projects into the cavity of the vesicle a flask-shaped mass of cells, the long axis of which lies in the same direction as the long axis of the vesicle, and measures 0·09 millim.; in breadth it measures 0·04 millim.

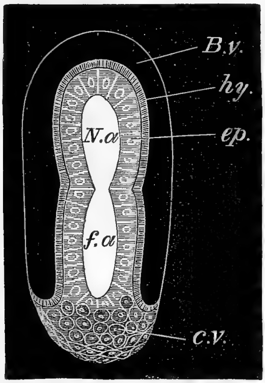
It will thus be seen that this mass of cells fills up the greater part of the cavity of the vesicle, and the placental pole of the mass is continuous with, and forms part of, the wall of the blastodermic vesicle. The flask-shaped mass of cells inside the vesicle, and which has its origin at the placental pole, is formed of cells, the subsequent history of which shows that they are mainly epiblastic with a covering of hypoblast.

The epiblast cells, *e.p.*, are roughly circular, approaching the columnar character in form, and at the placental pole of the ovum lie in contact, if not actually continuous, with this limited area of the wall of the blastodermic vesicle. They are covered on the side next the cavity of the blastodermic vesicle by a single layer of hypoblast cells, *hy.*, which also at the placental pole of the ovum lie contiguous with the wall of the blastodermic vesicle at the margin of the epiblast cells.

The vesicle, then, at its placental pole exactly resembles the germinal area of an ordinary vesicle, such as that of the rabbit, which, taking the widely accepted description, is formed of a protective covering of cells (Rauber's layer of "Deckzellen") continuous with the wall of the blastodermic vesicle; below this a layer of epiblast cells, while still more internal is the hypoblast layer. These only extend over a limited area of the wall of the vesicle, and it is still a disputed question whether the epiblast forms the wall of the vesicle, or whether this is formed by the covering layer. The vesicle above described for the rat has all the elements described for the rabbit with this difference, and herein lies the whole explanation of this form of development—that there is an enormous development of the epiblast cells, which are thrust, as it were, into the cavity of the vesicle and carry the hypoblast as a single layer of cells over their surface. This early stage, in which the epiblast is solid and is *not* in the form of a cellular layer, is succeeded in the course of a single day by a stage in which the solid epiblast of the preceding stage is converted into an epiblastic vesicle by the formation of a cavity in the interior of the solid mass. (Fig. 2.) This epiblastic vesicle is not open either at the placental or free pole of the ovum, and it has to be now noted that the embryonic area is not formed at the placental pole of the ovum, where the epiblast cells began first to multiply and protrude into the cavity of the blastodermic vesicle, but adjacent to the free pole of the ovum, which the epiblast, owing to its great development, has now almost reached (covered of course by the hypoblast layer).

At this stage, in addition to the changes in the solid mass of epiblast, leading to the formation of a cavity in its centre, there has also been a development of cells from the placental pole of the blastodermic vesicle, which go on increasing during the course of development, and which ultimately form a layer of large cells lying next the maternal decidua. (Fig. 2, *c.v.*) In the course of another day the last-mentioned stage is succeeded by one in which the single epiblastic vesicle becomes

FIG. 2.



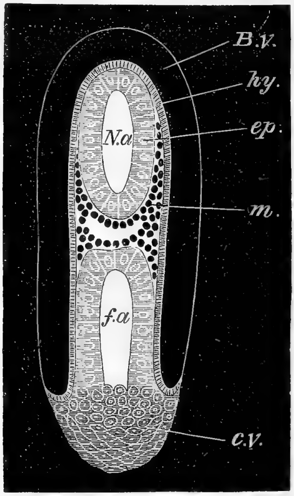
Section of ovum from rat, 7 days 16 hours.

nipped in the middle by a circular constriction, the edges of which ultimately meet, and thus the once single epiblastic cavity is now divided into two (fig. 3). One at the free pole of the ovum becomes the neuramniotic cavity, *N.a.*, the other next the placental pole of the ovum is what may be termed the false amnion cavity, *f.a.*

It is at this stage that the mesoblast first makes its appearance, budding off from the epiblast at the hinder end of the embryonic area, and spreads rapidly over this area in the form of two lateral plates, which are not continuous across the middle line; but in addition to this embryonic portion, the mesoblast also spreads in another direction, splitting at the anterior and posterior ends of the embryonic area. One part of it passes over the amniotic part of the neur-amniotic cavity, the other passes internal to the hypoblast, over the free surface of the epiblastic wall (that surface adjacent to the true amnion) of the false amnion cavity. (Fig. 3, *m.*)

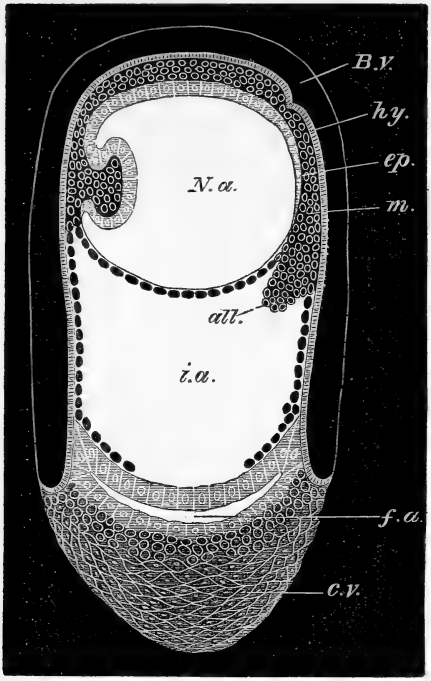
This stage in the course of another day is succeeded by one in which the two halves of the epiblastic vesicle are separated by a space, which Dr. Thomson suggests, may be termed the interamniotic space (fig. 4, *i.a.*), while the upper wall of the false amnion cavity has begun to be depressed towards the lower, encroaching upon and filling up its own cavity. (Fig. 4.) This encroachment goes on more and more until finally the walls approach and fuse with one another, the cavity disappearing, and the walls of the false amnion are converted into a

FIG. 3.



Section of ovum from rat, 8 days 16 hours.

FIG. 4.



Section of ovum from rat, 9 days 17 hours.

solid mass of cells, which forms a considerable part of the structure of the fœtal placenta, and which comes into relation with the allantois in a manner to be presently described.

The allantois consisting of a solid mass of mesoblastic cells, in which there is no hypoblastic dilatation, grows outwards from the posterior end of the embryo into the above-mentioned inter-amniotic space. (Fig. 4, *all.*) A little over the eleventh day it reaches the mass of cells formed from the false amnion, while its vessels formed in the same manner as in other animals, pass through the mass of cells and penetrate the maternal part of the placenta, accompanied by extensions of the cellular structure derived from the placental pole of the blastodermic vesicle. The alimentary canal begins to be separated from the general hypoblast in the form of a groove which up to the twelfth day opens freely into the space between the hypoblast, and the wall of the blastodermic vesicle. The peripheral portion of hypoblast outside the alimentary groove, which is equivalent to the yolk sac of other animals, and becomes vascular in a similar manner by the mesoblastic lining on its inner surface, becomes thrown into villous processes which are continued over the surface of the placenta as far as the central place of insertion of the allantois.

The arrangement of the fully formed membranes is now easily seen: the wall of the blastodermic vesicle is the external one, and is continuous with the margins of the placenta; it becomes much stronger in the older stages. Inside this membrane is one formed of hypoblast and vascular mesoblast and which was continuous with the alimentary canal, while inside the interamniotic space is the stalk of the allantois.

I do not at present enter into a further description of the general phenomena of fœtal development, because after the closure of the neur-amniotic cavity and the establishment of the relation of the parts already described, the subsequent phenomena are essentially similar to those of other animals.

As to the cause of the peculiar form of development of the blastodermic layers in some of the rodents to which reference is made in this communication, it would be premature to speculate until its conditions have been more fully ascertained; but I would remark that this peculiarity appears to stand in some close and constant relation to the very early, rapid, and voluminous formation of the solid mass of decidua within which the ovum is from the first enclosed in all those animals in which the so-called inversion of the layers has been observed.

Since the observations above recorded were made, three short papers bearing upon the subject have appeared in Germany during the month of November, one by Professor Hensen, of Kiel,* dealing with the guinea-pig, another by Professor Kupffer, of Munich,† on the field mouse, and a third by Professor Selenka, of Erlangen,‡ on the white mouse, all of which were unknown to me until some time after I had arrived at the conclusions stated in the preceding pages. Without entering into any detailed criticism of the contents of these papers, I may state that the main difference between Professor Kupffer and myself has reference to the early condition of the epiblast, which is solid in the rat, but which he describes as forming in the field mouse a single involuted layer.

In conclusion I have to express my warmest acknowledgments to Dr. Thomson, who has gone over all these observations with me, and to whom I am indebted for many suggestions in my description of this work.

The figures are diagrammatic.

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* "Verhandl. des Physiol-Vereins in Kiel." Sitzung vom 2 Nov., 1882.

† "Sitz. Ber. d. k. B. Akad. der Wiss." 4 Nov., 1882.

‡ "Biolog. Centralblatt." 15 Nov., 1882. P. 550.

Alphabetical List of Reference Letters.

All.	Allantois.
B.v.	Blastodermic vesicle.
c.v.	Cellular layer derived from placental pole of blastodermic vesicle.
ep.	Epiblast.
f.a.	False amnion cavity.
hy.	Hypoblast.
i.a.	Interamniotic space.
m.	Mesoblast.
N.a.	Neur-amniotic cavity.
p.p.	Placental pole of blastodermic vesicle.

VII. "On the Electric Discharge with the Chloride of Silver Battery." By WARREN DE LA RUE, M.A., D.C.L., F.R.S., and HUGO MULLER, Ph.D., F.R.S. Received December 21, 1882.

In anticipation of a paper to be shortly communicated to the Society, we wish to state that we have found that the pressure of least resistance for a given gas is not a constant, but that it varies with the diameter, shape, and dimensions of the vessel employed.

Moreover, that the dark space near the negative in electric discharges in vacuum tubes is dark only by comparison; for we have obtained a photographic image of the dark discharge in a tube in which the strata remained steady during forty-five minutes. The time of exposure was fifteen and thirty-five minutes; a comparison of the latter result with a photograph obtained of the strata in two and a half seconds shows that the dark space is 840 times less bright than a stratum.

Lastly, a tube with palladium terminals, which we made several years ago, containing hydrogen gas, shows in a remarkable manner the power of terminals to occlude gas and to give it off again. On passing an electric discharge through this tube for a few seconds, it becomes blackened, especially near the negative, by the deposit of a mirror-like film; on leaving the tube for few days' rest, this mirror disappears entirely, and is reproduced by passing a fresh current. It is most probably a volatile hydrogen alloy of palladium. The effects described have been reproduced very many times during eight years.

The Society adjourned over the Christmas Recess to Thursday, January 11th, 1883.

January 11, 1883.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Experiments, by the Method of Lorentz, for the further Determination of the Absolute Value of the British Association Unit of Resistance, with an Appendix on the Determination of the Pitch of a Standard Tuning-Fork." By Lord RAYLEIGH, F.R.S., Professor of Experimental Physics in the University of Cambridge, and Mrs. H. SIDGWICK. Received December 8, 1882.

(Abstract.)

The experiments described in the present paper were carried out during the spring and summer months of the present year, at the Cavendish Laboratory, and are divided into three distinct series. In the first and second series, the induction coils were situated nearly in the plane of the revolving disk, as in Lorentz's original use of the method; the difference between the two series relating only to the speed of rotation, which was varied in the proportion of 10 : 16. The third series presents a point of novelty, in that the induction coils were separated from the disk to such a distance as to render the accuracy of the result practically independent of the mean radius of the coils.

The small resistance, traversed by the battery current, to which the terminals of the galvanometer branch are connected, was obtained indirectly by a method of shunting. Thus in the first series, the principal part of the battery current passed on one side through two unit coils, placed in multiple arc, and equivalent to $\frac{1}{2}$, and only a comparatively small remainder through a second branch, composed of two coils in series, of values 10 and $\frac{1}{10}$. The terminals of the galvanometer branch were connected with the extremities of the $\frac{1}{10}$, and the difference of potentials between them, due to the primary current, was thus reduced to that which would be required to drive the current through a resistance of $\frac{1}{200}$.

From the first series—

$$1 \text{ B.A. unit} = \cdot 98674 \times 10^9 \text{ C.G.S.}$$

From the second series—

$$1 \text{ B.A. unit} = \cdot 98669 \times 10^9 \text{ C.G.S.}$$

From the third series—

$$1 \text{ B.A. unit} = \cdot 98683 \times 10^9 \text{ C.G.S.}$$

As a mean we take—

$$1 \text{ B.A. unit} = \cdot 98677 \times 10^9 \text{ C.G.S.}$$

With use of the ratio between the mercury unit and the B.A. unit, found by us ("Proc. Roy. Soc." May, 1882) this gives—

$$1 \text{ mercury unit} = \cdot 94150 \times 10^9 \text{ C.G.S.},$$

or, which is the same thing, the ohm is the resistance of a column of mercury at 0° Cent., whose section is one square millimetre, and whose length is—

$$1062 \cdot 14 \text{ millimetres.}$$

The very close accordance between the result of the present investigation, and that obtained by the method of the revolving coil (·98651), and by Glazebrook (·98665), using another method again, leads us to hope that no error of importance can have escaped detection.

The Appendix is devoted to a record of experiments having for object the determination of the absolute pitch of a certain tuning-fork, which has served as the standard of time throughout all our work upon this subject. It is believed that the method employed is worthy of attention, and may be useful to other physicists.

II. "On the Skeleton of the Marsipobranch Fishes. Part I. The Myxinoids. (*Myxine* and *Bdellostoma*.) By W. K. Parker, F.R.S. Received December 14, 1882.

(Abstract.)

At present nothing is known of the development of these remarkable fishes, but their structure in the adult state is of great interest, and as the other related type—the Lamprey—has had great attention given to it lately, in most of its stages, I have thought it would be profitable to anatomists to have a detailed account of the skeleton in these lower and less known types. I received several specimens of the adult Hag-fish (*Myxine*) from my friends the late Professor Rolleston, F.R.S., and Mr. Frank Buckland; for fine specimens of the gigantic type (*Bdellostoma*) I am indebted to Professor Ray Lankester, F.R.S.

My guide in this work has been the excellent and most accurate Johannes Müller—his four memoirs (well known to anatomists) on the Myxinoids and related types have been absolutely necessary to me. I shall be proud if this and the next paper are thought worthy of being considered an *appendix* to his incomparable works on these types.

But as regards the Marsipobranchs generally, especially the Lamprey, I am deeply indebted to Professor Huxley's writings and to discussions with him upon these fishes; and in the same way to the late Professor F. M. Balfour, F.R.S.; and I am indebted further to my young friend Mr. W. B. Scott, of Princeston, U.S., who, after Calberla, has worked largely on the early development of the Lamprey.

What I have been able to make out with regard to the skeletal parts of the Lamprey will be offered to the Royal Society very soon, and then the structure of the adult Myxinoids and of the various stages of the Petromyzoids can be compared together.

But the various kinds of the "Anurous Amphibia"—hundreds of species—give us, in their larval state, a sort of temporary generalised Marsipobranch fish; it is not unknown that I have given several years of labour to these types, and I feel now that I may, with caution, attempt to explain the morphology of the skeleton in all these three related groups—the *Myxinoids*, the *Petromyzoids*, and the *Anura.*

However far apart, *now*, these three groups may be, they are seen to be the nearest of kin to each other when we consider the other "Ichthyopsida." Moreover, they form a curious *scale*, so to speak, one rising above the other in a regular order; for the Myxinoids are a sort of arrested *Ammocœte* or larval Lamprey, and the Lamprey in its adult state is quasi-larval if it be compared with the anurous amphibian—Frog or Toad.

The Myxinoids are very anomalous, and this is seen even in their histology; in them, as in the Lamprey, there are two kinds of cartilage—one very dense and almost as hard as bone, and the other soft, like the cartilage of young embryos of higher types.

But in the Myxinoids one very large bar, the great basi-branchial, is formed of a light, elastic, *vacuolar* tissue, but little denser than that of their great persistent non-segmented notochord, and, like it, ensheathed in a very thick web of fibrous or tendinous tissue.

I suspect that this fact will have a meaning for the student of the lower *non-craniate* "Chordata"—*Amphioxus*, the Ascidians, &c.

In the wide-mouthed, *non-suctorial* larvæ of the Cape Toad (*Dactylethra*)—I found the whole chondro-skeleton composed of a peculiar kind of cartilage intermediate between hyaline cartilage and this vacuolar tissue of the Myxinoids; it is more like the pith of a plant than like ordinary cartilage.

In their cranio-facial skeleton the Myxinoids are very remarkable; where segmentation is perfect in other piscine types they only exhibit a lattice-work of continuous growth; in the median region of the skull-base, where other types show but little or only temporary distinctness of parts, these fishes develop and retain large independent cartilages.

The lamprey has a large superficial basket-work of soft cartilage (*extra-branchial*), and its gill-pouches keep related to this and to the rest of the structures of the mouth and throat. But in the Myxinoids the basket-work is *intra-branchial*, and corresponds to the system of segmented arches of the higher Cartilaginous, the Ganoid, and the Osseous fishes. But these non-segmented arches soon lose all relation to the branchial pouches, which are removed so far backwards that they begin under the *twentieth myotome;* whilst the end of the pericardium is under the *fortieth.*

In seeking light upon the primordial condition of the Vertebrata, one naturally looks to such forms as the Myxinoids. For in these types, even in the adult state, there are neither limbs nor vertebræ, and no distinction between head and body, except the beginning, in the head, of a cartilaginous skull—a *continuous structure*—not showing the least sign of secondary segmentation, and by far the greater part of it in front of the notochord, or axis of the organism. But here our *gradational* work agrees with the *developmental*, for the continuous skull-bars constantly arise *before* the secondary cartilaginous segments that are found between the myotomes behind the head. Evidently, therefore, the early "Craniata" grew supports to the enlarged and subdivided front end of their neural axis, long before any structures beyond strong fibrous septa were developed between the muscular segments of the body. As for the linear growth, the greater or less extension backwards of the main organs—circulatory, respiratory, digestive, urogenital—that, in the evolution of the primary form, was a thing to be determined by the "surroundings" of the type. "Thereafter as *they* may be" was the tentative idea in this case.

Certainly, in the Marsipobranchs, and in their relations, the larval "Anura," we have the most archaic "Craniata" now existing; in these the organs may be extended far backwards in a vermiform creature, as in these low fishes, or kept well swung beneath the head—the body and tail together forming merely a propelling organ, as seen in Tadpoles, especially the gigantic Tadpole of *Pseudis.*

Thus we see that in low limbless types there is no necessity for the development of more than fibrous "metameres" in the spinal region; but the vesicular brain, the suctorial lips, the branchial pouches, and the special organs of sense—these all call for support from some tissue more dense than a mere fibrous mat or web. In the *Myxinoids* we see that *four* special modifications of the connective tissue series are developed for the support of the properly *cephalic* organs, and for them only; thus these fishes are *Craniata*, but are not *Vertebrata;* that is, if we stick to the letter, which, of course, we do not.

At first some disappointment is felt, after careful study of these types, for, notwithstanding the low level in which they remain—they are mere specialised *Ammocœtes*, keeping on the same "platform" as the larval Lamprey;—yet some parts of their organisation do undergo a marvellous amount of transformation, and are, indeed, as much specialized in conformity with their peculiar habits of life *as any Vertebrates whatever, the highest not excepted.*

Yet, on the whole, the Myxinoids are a sort of *Ammocœtine* type, whilst the transformed Ammocœte, the adult Lamprey, comes nearest to the untransformed Frog or Toad—the *Tadpole.* But the mere putting of this shows (suggests at any rate) what *losses* the Fauna of the world has sustained during the evolution of the Craniate forms; *now*, the Myxinoids, Petromyzoids, and numerous Amphibia must all be kept "within call" of each other; but the types that have been culled out between them cannot be numbered. Some other kinds of fish are evidently the descendants of primordial "Marsipobranchs," notably *Lepidosteus*, the development of which has been lately studied, and the results of which are being published in the "Philosophical Transactions." But the *Chimœroids*, *Dipnoi*, and, still more important, the *Myxinoids*, themselves, have still to be followed through their early stages. If the present paper is of any value to the morphologist, one on the embryology of these low forms would be worth much more.

The Myxinoids keep on the low "platform" of the larval Lamprey (*Ammocœte*) in the following particulars, namely:—

a. The notochord has no paired cartilaginous vertebral rudiments in the spinal region.

b. The trabeculæ end in the ethmoidal region without growing forwards into a cornu (or *two continuous* cornua).

c. There are merely "barbels" round the mouth; no *labial cartilages.*

d. The last character involves this, namely, that the special armature of horny teeth, attached to the labials in the adult *Petromyzon*, is absent.

e. The organs of vision are very feeble, and probably almost useless; in the *Ammocœte* they are arrested for a time.

f. The cranium is a mere *floor*, without side-walls or roof.

The Myxinoids come near to the adult Lamprey in the following particulars, namely:—

a. There are developed outside the skull proper, but not segmented from it, palato-quadrate and hyoid cartilages.

b. There is a very large median cartilage belonging to both the hyoid and branchial regions.

c. The cranium acquires a floor by the development of a special "hinder intertrabecula."

d. There is a large median cartilaginous olfactory capsule.

The Myxinoids go beyond even the adult Lamprey in the following particulars, namely :—

a. The facial basket-work is much more perfect; and as this is a generalised condition of the true *intra-visceral* system of cartilages, it is a very important character; there is not only a development of the "suspensorium," equal to that of the Lamprey, but the *suspensorial part* of the hyoid is developed also (it is suppressed in the Lamprey); and there is, in *Bdellostoma*, a large complete first branchial arch, and in both kinds pharyngo-branchial rudiments of the second branchial arch.

b. The respiratory (branchial) pouches are much more specialised by being carried far back under the spine.

c. There is not only a distinct sub-cranial intertrabecula, but also a large pre-cranial or nasal median cartilage of the same nature.

d. The opening of the median olfactory sac is not a mere short membranous passage, but a long tube, encased in a series of cartilaginous (imperfect) rings.

e. Correlated with the non-development of the suctorial labial cartilages, there is an enormous development of the lingual, this basal bar becoming not only double, but in front quadruple, and the "supra-lingual" cartilages, which are very small in the Lamprey, and carry only one pair of rows of small second teeth, are in the Myxinoids very large, and carry two pairs of rows of large teeth, with the addition of a median antagonistic "ethmoidal tooth."

Lastly, the greater development of the intra-visceral (= "intra-branchial") cartilages is correlated with the suppression of the extra-visceral basket-work seen both in the larval and adult Lamprey, and also in the larvæ of the "Anura," generally.

Morphologists must kindly accept this piecemeal work of mine; it will take on a form, or frame, some day; but much of the materials for its completion are still wanting, and, when obtained, the working of them out must not be done hurriedly.

I may remark, that having just now to work at "the extremity of both ends" of the Vertebrata—the Marsipobranchs and Mammalia —I find the former very excellent as carriers of light to the latter. Everyone will see that by far the greater bulk of the existing Vertebrata are very specialised, each order and class on its own lines, and that any creature to be like a primordial mammal must be very generalised or archaic.

III. "The Direct Influence of Gradual Variations of Temperature upon the Rate of Beat of the Dog's Heart." By H. NEWELL MARTIN, M.A., M.D., D.Sc., Professor in the Johns Hopkins University, Baltimore, U. S. A. Communicated by Dr. M. FOSTER, Sec. R.S. Received December 27, 1882.

(Abstract.)

In the investigations described, the method of experiment was such as to completely isolate physiologically the heart of the dog from all the rest of the body of the animal, lungs excepted.

This was accomplished by occluding the right and left carotid and subclavian arteries, the aorta just beyond the origin of the left subclavian, and ligaturing both venæ cavæ and the azygos vein. In consequence the only fraction of the systemic circulation left open was that through the coronary system of the heart; no organ but the heart itself has any blood sent it, except the lungs. Hence the cerebro-spinal nerve-centres and the sympathetic ganglia very soon die, while the heart remains alive, in good working condition, for two hours or more. The right auricle is supplied uniformly with defibrinated calf's blood, conveyed to the superior vena cava from Marriotte flasks. The blood, after traversing the pulmonary circuit, is finally pumped by the left ventricle into a cannula, which is tied into the aorta just beyond the origin of the left subclavian artery. From the distal end of the cannula a wide rubber tube carries the blood to an exit cannula seven or eight feet above the level of the heart. By raising or lowering this exit, and by raising or lowering the level of the Marriotte flasks feeding the heart, arterial and venous pressures could be changed at will, or maintained very nearly constant.

Venous and arterial pressures being kept constant, the temperature of the blood supplied to the heart was gradually changed by raising or lowering the temperature of the water contained in the vessels in which the feeding Marriotte flasks were immersed.

The pulse rate was recorded by a Fick's spring manometer, and arterial pressure by a Marey's mean-pressure mercury manometer, each being connected with the central stump of a carotid artery. Temperatures were read by means of a thermometer tied into the root of the left subclavian, so that its bulb projected into the aortic arch.

Uniform artificial respiration was maintained.

As the result of many experiments it was found (1) that the isolated dog's heart beats quicker when supplied with warm blood, and slower when cold blood is supplied to it; (2) that the rate of beat depends much more upon the temperature of the blood in the coronary arteries than on its temperature in the right auricle or ventricle; (3) that when defibrinated calf's blood is used to feed the heart that organ

cannot be kept alive as long as when defibrinated dog's blood is employed; (4) that no matter how long an experiment lasts the defibrinated blood, circulated again and again through heart and lungs, shows no tendency to clot; hence fibrinogen is not produced in those organs.

The question answered by the first of the above results was the one for whose solution the research was undertaken. The experiments show that, in spite of its highly developed extrinsic nervous apparatuses, the heart of the mammal does, so far as its rhythm is concerned, in its own nervo-muscular tissues, respond to temperature variations within wide limits (42°—27° C.), just as the frog's heart or that of the embryo chick does. To account for the quick pulse of fever we, therefore, need not look beyond the mammalian heart itself; we require no theoretical assumption of any paralysis of inhibitory, or any excitation of accelerator cardio-extrinsic nerve-centres.

January 18, 1883.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "Preliminary Paper on a Uniform Rotation Machine; and on the Theory of Electromagnetic Tuning Forks." By R. H. M. BOSANQUET, St. John's College, Oxford. Communicated by Professor H. J. S. SMITH, F.R.S. Received December 20, 1882.

(Abstract.)

The primary object of the machine is the construction of standard notes. It admits also of the accurate determination of tuning forks, &c., having pitch near that of any standard note of the machine, besides other applications.

The machine consists of a three-crank axle with a fly-wheel. The cranks are acted on by electro-pneumatic levers, the valves of which control the wind supply as the slide-valve of a steam engine does the steam. Two of these are acted on by a commutator on the axis; the third is connected with a clock which closes the circuit at every second. The effect of this is to govern the machine, so that it will

revolve once per second under wide variations of the driving power. The fly-wheel originally consisted of, and now carries, brass disks two feet in diameter, having various numbers of slits cut in them spokewise. Those disks which have been made represent pitches of tenor C, covering the whole range in practical or theoretical use.

Tuning forks having any multiple or submultiple of the frequency of the disk employed, present, when examined against an illuminated background behind the disk, toothed patterns which remain stationary. If the fork is a little sharp the pattern moves in the opposite direction to the slits, if flat in the same direction. When the machine oscillates about its mean velocity, a fork in tune with the machine presents a pattern which swings backwards and forwards.

While the machine was yet in an early and imperfect state, it was demonstrated that large tuning forks, when excited electrically with a mercury contact, vary their frequency by change of level of the mercury, within limits which may readily amount to 1 in 200.

A device due to Lord Rayleigh was employed with the object of extinguishing the oscillations. A ring of metal tubing was filled with water, and mounted as a rim on the disk. But this had practically no effect. An extensive series of experiments was then undertaken, in which large quantities of tubing filled with water were employed. Some slight effect was ultimately obtained, but nothing useful. The end desired was attained with mercury in india-rubber tubes. The damping does not as yet amount to a dead beat action, and it is hoped that it may be improved. The damping at present obtainable under practical conditions is reduction of swing to $\frac{1}{2}$ in 10^s.

With the machine thus improved, observations have been made of the variations of pitch of a middle C fork, with an electric spring contact, under varying tension. The changes which occur appear to be less than in the case of the mercury contact.

The experiments that have been made on electromagnetic tuning forks lead to an outline of the theory of the motion in this case, which appears to depend on the time of magnetization of the magnetic system. Although studies have been made on this subject, there is nothing in the shape of a theory depending on absolute measure for guidance in arranging and interpreting experiments. Such a theory is here outlined, and compared with experiment.

The theory depends on the assumption that the systems dealt with are closed magnetic circuits with small breaks, or cores of solenoids not projecting far from the coils. Under certain limitations the resistance of the magnetized iron can be neglected in comparison with that of the air spaces traversed, according to the values of the permeability of iron furnished by Rowland's experiments.

The permeability of the system is then deduced from the comparison of resistances in the air space of the core of the solenoid, and in the air spaces which form the breaks in the magnetic circuit.

The coefficient of electromagnetic momentum is then, by means of known equivalents, expressed in terms of the permeability of the system, and thence, according to the above result, in terms of the configuration of the breaks in the magnetic circuit.

A method is then developed of calculating the coefficient of electromagnetic momentum from the observed mean current during the excitation of a tuning fork of known period.

Four experiments of this description were made, and the value of the terms depending on the configuration of the air spaces analysed and interpreted in connexion with the numerical results thus obtained.

In this manner I was led to distinguish two theoretical cases which were connected by an empirical configuration formula, bridging over the gap between them. The experiments in question agree with the configuration formula to a degree far beyond what could have been anticipated, considering the roughness of the methods employed for the determination of the several elements concerned.

The result is, that in a certain class of electromagnetic systems, when the configuration is given, the permeability and coefficient of electromagnetic momentum can be approximately assigned, and the whole electromagnetic behaviour of the system approximately calculated.

With this theory at my disposal I hope to make further contributions to the knowledge of electromagnetic tuning forks.

II. "On the Skeleton of the Marsipobranch Fishes. Part II. The Lamprey." By W. K. Parker, F.R.S. Received January 10, 1883.

(Abstract.)

In working out this type I have been greatly indebted to the labours of J. Müller, Huxley, Schneider, Balfour, and Scott.

For *materials* I am indebted to two of the above-mentioned anatomists, namely, Professor Huxley and the late Professor Balfour, also to Surgeon-Major Francis Day, of Cheltenham, and Osbert Salvin, Esq., F.R.S.

The transformed skeleton is described in various young individuals of the Sea Lamprey (*Petromyzon marinus*), from four to eight or nine inches in length. The smallest of these was scarcely through its metamorphosis. A specimen of *P. planesi* was worked out at the same

stage. The various sections of the adult were made from the larger river species, *P. fluviatilis*, so also were the various larval specimens; but the embryos were of the small kind—*P. planesi.* These were reared by Messrs. Salvin and Balfour.

I have first described the skeletal structures after metamorphosis, as their condition then is best known to anatomists. I then explain what is seen in the embryo, and after this the larval or *ammocœtine* stage. If my friends are successful in obtaining for me larval Lampreys actually transforming, a Third Part, a *much smaller* paper, will be prepared.

In spite of the invaluable help I have received from my fellow-workers, my task has not been an easy one; it has been taken up again and again, after research into the development of other fishy types.

The suctorial mouth has its highest development in the Lamprey; in the Myxinoids (*Myxine* and *Bdellostoma*) there is no circular disk with horny teeth, but merely an oral fissure, surrounded by barbels, and having inside it a huge tongue beset with two oblique rows of recurved and inturned horny teeth, antagonised by a single ethmoidal tooth. In the larva of the Lamprey the mouth is not circular, and the lower lip is far back, covered by the upper, which is like a hood; there are no teeth of any kind, only moss-like "barbels" or *papillæ* under the upper lip.

In the Tadpole the mouth is suctorial, the lower lip being converted into an imperfect ring, which is completed by the upper lip. Here the cartilage of the lower lip is not a perfect ring, as in the Lamprey, but is in two parts, and is formed into a sort of *horseshoe.* Inside this compound ring there are sharp horny plates or teeth, and the folds of the lips, all round the mouth, are covered with a horny rasp.

Correlated with the perfectly suctorial *lower* lip of the Lamprey, which is a *post-oral* structure entirely, we have the most perfect form of the superficial branchial skeleton, a basket-work of soft cartilage, which appears in the early embryo, and only gains enlargement, fore and aft, with all its snags and outgrowths, after metamorphosis. Besides this there are no rudiments of *internal* branchial arches, such as we find in the Tadpole. The only parts developed *inside* the head-cavities and branchial arches are the generalised and rudimentary mandibular and hyoid arches. In the Tadpole there is no *pier* to the hyoid arch, and the *first cleft* is arrested as a small blind pouch; this state is persistent in the Lamprey. But, after metamorphosis—the lingering latter part of that profound change of structure—the young Frog and Toad acquire a pier to their hyoid arch, right and left. This, however, does not become functional to the arch, much less assist in supporting the mandible, as a "hyomandibular," but is transformed into an osseo-cartilaginous chain—a *stapedio-incudal* series, specialised correlatively with the expanded rudiment of the first cleft, now enlarged into a *cavum tympani*, with a large "Eustachian" opening. The little mandibles of the Tadpole, which served as arms to carry the divided suctorial disk, and lay across the fore face, become very long, and are often hinged on to their pier behind the occiput, and the cartilages of the suctorial disk straighten out and add to the length of the lower jaw in front. These things show how this *temporary* "Petromyzoid," the Tadpole, blossoms out into unthought-of specialisations, and becomes a *quasi-reptile*, worthy of a place far above the Lamprey, and even far above all other *Ichthyopsida*.

The Myxinoids never gain the level (or platform) of the adult Lamprey or the larval Frog; they acquire no rudiments of vertebræ —only a huge notochord—uniform or non-segmented. But then their *lingual teeth*, rudimentary in the Lamprey, and not present in the Tadpole, are very large, and have a large *buccal* skeleton of their own. They have no *extra-branchial* basket-work, but do develop at least *four* visceral arches, the hyoid (or second) being very large and perfect, but not segmented as in higher fishes. Everything is in a generalised state. But the *first arch* has no lower jaw developed on it, its *lower* part is arrested, and the *two* or *three* proper gill-arches are dissociated from the gill-pouches, which are carried far back, under the spine. I must refer to the main paper (Parts I and II) for details, but I feel sure that every morphologist will agree with me when I assert that these three related, but widely separated groups—the Myxinoids, Petromyzoids, and Anura, are worthy of all the attention that anatomists have given to them, and that if ever we come to see how the Vertebrata have arisen, during time, from *chordate* forms on a lower platform, we shall have to question and cross-question these Marsipobranchs—not once nor twice, but many times.

For myself, I shall be grateful if this limited contribution to the anatomy of the Marsipobranch fishes should draw the attention of other workers, and attract them to this fruitful field of research.

III. "On the Infectivity of the Blood and other Fluids in some Forms of Septic Disease, and the reputed occurrence therein of an Increase of Virulence in Successive Inoculations." By G. F. DOWDESWELL, M.A. (Cantab.), F.L.S., F.C.S., &c. Communicated by Dr. M. FOSTER, Sec. R.S. Received January 15, 1883.

The remarkable fact that in some cases the blood of an animal, intoxicated with putrid matter, becomes itself "infective," capable of reproducing in other animals those symptoms which have occurred in it, though previously asserted by others, was first conclusively demonstrated in 1866 by MM. Coze and Feltz in France,* who further stated that in successive inoculations from one animal to another of the same species, such blood acquires a progressive increase of virulence, becoming more toxical than putrid matter itself, and that death follows inoculation with increasing rapidity. These observations were confirmed and greatly extended by Davaine,† in long investigations; hence the form of septichæmia here in question has since been known by the name of the latter observer. The first-mentioned writers founded their assertion of an increase of infective virulence upon the alleged fact that in successive inoculations‡ the incubation period was progressively shortened; whereas Davaine sought to show, by numerous experiments, that in the same manner successively smaller quantities of blood were required to create infection; that whereas to originate it some drops were necessary, ultimately, after several generations of transmitted infection, the billionth (in the French notation the trillionth) part of a drop, or less, was sufficient; but he regarded, indeed defines, septichæmia as a putrefaction of the blood in the living animal, a view naturally encouraged by the decomposition which in these cases occurs so rapidly after death.

Davaine's experiments were repeated by many observers, both in France and elsewhere.§ It is remarkable, however, that they one and all contented themselves with merely reproducing his earlier observations, inoculating in succession several animals with constantly diminishing quantities of blood, but without making any control experiments to ascertain whether in the first stages the blood was not already infective in the degree supposed to be attained only after several generations, and overlooked the fact that, in his later writings|| Davaine had qualified his first statements by showing that a maximum of infectivity was attained in the earlier generations. These conclusions and the theory of an increase of infective virulence in successive inoculations have since been generally accepted; but the question here

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* "Recherches Cliniques et Expérimentales sur les Maladies Infectieuses, &c.," Strasbourg, 1866, and Paris, 1872.

† "Comptes Rend. de l'Acad. des Sc.," Paris, Feb. 1, 1869, *et passim.* Also "Bull. Acad. Méd.," Paris, 1872, p. 907, &c.

‡ Or, as it was termed, "generations," with reference to the microphytes found to be present in the blood of these cases, which were regarded as constituting the active contagium, and which by propagation in the blood of living animals, it was said, acquired "renewed vigour" in successive generations. I have retained this term "generations" as convenient, but without here implying anything more than successive inoculations from one animal to another.

§ As by H. Dreyer, "Archiv. f. Expt. Path. u. Pharm." 1874, Bd. II, s. 149, &c., and Clementi and Thin.

|| *Loc. cit.*, "Bull. d. l'Acad. de Méd.," 1873, p. 124, &c.

involved has recently acquired fresh importance in reference to the relations of the lower fungi to disease, and the occurrence in them, as lately asserted by Dr. Hans Büchner,* P. Grawitz,† and others, of a transmutation of physiological species under altered conditions.

In observations recently made upon septichæmia in the mouse,‡ and other experiments, I obtained results which seemed to me to negative the occurrence of any progressive increase of virulence in the blood in successive inoculations, or of any transformation, physiological or morphological, in the organisms present, an opinion which was confirmed and the question discussed by Dr. R. Koch,§ in a work recently published.

In the case of the mouse, from the small size of the animal and the blood in most cases being more or less coagulated when examined, it was not easy to obtain accurately measured minute fractional quantities, by successive dilutions, for the purposes of experiment; for which reason, and also as it was with reference chiefly to the blood of the rabbit that the statements had originally been made, I selected that animal for the investigation of this question by the following methods.

To originate infection in the same manner as in other experiments on the same subject, putrid blood was used, generally that of the ox; but I have also employed that of the pig, sheep, &c., or of the rabbit itself, all with similar results. Of such blood, diluted with an equal bulk of normal saline solution and strained, a few drops were injected into the subcutaneous tissue of the back or abdomen, by means of a Pravaz syringe. In the subsequent high dilutions five or ten drops were generally injected, and these comparatively large quantities were employed in order that there might be less chance of error in the quantity of blood actually received by the animal, than would be the case with smaller injections, *e.g.*, of one drop, as used by Davaine; though his method probably reduces the liability to formation of abscesses at the site of injection, with the accompanying failure of infection, which, however, only occurred to me in a few instances. In these experiments, where it was requisite to guard against the possibility of accidental infection or contamination, instead of a syringe I used a glass tube drawn out to a fine point; this is readily inserted under the skin of the back in young animals, still more so under that of the abdomen. When the quantity to be injected requires to be accurately measured, it is easy once for all to calibrate the tubing by the usual method, and extemporise a graduated pipette, containing a given quantity, between two marks of the file.

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* "Sitzber. k. B. Akad. Wiss. zu München," 1880.

† Virchow, "Archiv.," Bd. 81, H. 2, s. 355.

‡ "Qrly. Journ. Micros. Sc.," N.S., No. 85, January, 1882, pp. 66–75.

§ "Untersuch. ü. d. Ætiol. d. Wundinfectionskrankheiten, &c.," Leipzig, 1878.

The symptoms which are observed after the injection of small quantities of putrid blood into a rabbit, when primary infection occurs, are very constant, and similar in most essential respects to those in the subsequent cases of transmitted infection, excepting that the inflammation at the seat of injection is more severe and extensive in the former than in the latter case, owing no doubt to the comparatively large quantity of septic* matter used. The period of incubation too is here very variable, in accordance with the uncertain toxicity of putrid blood; its duration is usually from twenty to forty, or sometimes sixty, hours, but if in specific infection an animal survives the latter period, to my experience, it invariably recovers. Putrid blood, however, in the quantities here used may be toxical, whether fatally or not, owing to the chemical poison it contains in solution, the sepsin of Panum, Bergmann, and other writers, whereas in subsequent cases of transmitted infection, when infinitely smaller quantities of the blood itself are used,—the thousandth or the millionth of a drop or less,—it is either fatally infective or, where not so, no symptoms of disturbance can be recognised. In these latter cases the incubation period is remarkably constant, being in the great majority of instances from twenty to twenty-four hours.

Beyond an extreme coagulability, which in this specific disease I have found invariable, in this differing from the observations of others, and frequently a great increase in the number of the white corpuscles of the blood, I have not recognised any constant change in the characters of that fluid nor in the form of the red corpuscles, as described and figured by Coze and Feltz, and as stated by others, in this case; and it seems to me that the appearances there described are often rather those which occur in normal blood from the methods of preparation, exposure to the air, &c., than constant pathological features. These authors, however, describe (*op. cit.*, p. 67) filamentous processes developed from the red corpuscles, of the nature of which they are uncertain, and conjecture that they may be parasitical micro-organisms. This statement of theirs has not been noticed, as far as I know, previously. I have observed the same thing, as already recorded,† in the blood of septichæmic mice. These bodies, which I have investigated and fully described elsewhere,‡ are mere processes developed from the stroma of the red corpuscles. They may be produced artificially and are indicative of a pathological condition of the blood.

The rapid coagulation of the blood upon death, often within a very few minutes, rendered it necessary to obtain it with the least possible delay. In the latter cases, *i.e.*, of transmitted infection, where the period of incubation is constant within a few hours, it is easy to watch the death of an animal and examine it immediately; but in the case where it is originated by putrid matter, this period varies within comparatively wide limits, death perhaps occurring during the night or when not expected. It is then more difficult to obtain it before coagulation occurs, and hence several failures were experienced in endeavouring to prepare successively diminishing quantities of the blood of this generation by fractional dilution.

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* I here use the term "septic" in its proper signification of "causing putrefaction," or accompanying it.

† "Qrly. Journ. Micros. Sc.," *loc. cit.*, p. 69.

‡ *Ib.*, Vol. 25, January, 1881.

Experiments.

Infection was originated in a young rabbit, No. 1, after some previous failures, by injection under the skin of the back of 3·0 gtt. of putrid bullock's blood, diluted, the animal was found dead forty hours after inoculation, with the appearances mentioned as usually occurring in these cases. Around the site of injection was observed diffuse hyperæmia, with extravasation of blood from the small vessels, a marked induration and discoloration of the subcutaneous tissue at the same spot, which appearance I have found invariably whenever infection occurred. There was no œdema, nor, beyond congestion, was any change in the spleen or other organs apparent. The blood on examination was much coagulated; in that of the heart were found, though irregularly distributed, numerous micro-organisms, a form of bacterium hereinafter more particularly referred to. These were not found in this or subsequent cases in any of the organs or other tissues, but as they are minute, and even in the blood when unstained, are difficult to recognise, in other tissues they may easily escape observation. The case of this rabbit so intoxicated by putrid blood, I have, in accordance with the phraseology of Davaine and other previous writers, termed the first generation of infection, though obviously for the reasons referred to, it would be more strictly accurate to designate such animal, as itself, not infected but poisoned, and originating infection.

Another rabbit, No. 2, thereupon received in like manner by injection, one drop diluted of the blood of No. 1. It died within twenty-four hours; blood from its heart was immediately diluted to different degrees, and ten drops of the various dilutions were injected into five other rabbits, all of similar size and condition, so that they received respectively the following quantities of blood, viz.:—No. 3, $\frac{1}{10}$th gtt.; No. 4, $\frac{1}{100}$th gtt.; No. 5, $\frac{1}{1000}$th gtt; No. 6, $\frac{1}{10000}$th gtt.; and No. 7, $\frac{1}{100000}$th gtt. No. 3 died in 25 hours, No. 4 in 24 hours, No. 5 in 25 hours, No. 6 within 40 hours, and No. 7 in 27 hours; all, with the exception of No. 6, in my presence, and all with very similar symptoms; thus showing (1) that in this form of septichæmia the period of incubation is in nowise proportionate to the quantity used for inoculation, and (2) that in the so-termed second generation of infection, septichæmic blood is already infective in the $\frac{1}{100000}$th of a drop. Infection was then transmitted through the fourth, fifth, and sixth generations, by inoculation with similar small quantities and like results, the animals dying respectively within 23, 48, and 20 hours. The blood of this latter—the sixth generation—was diluted to different degrees, and ten drops of the solution injected into four other rabbits, which received—No. 11, $\frac{1}{100}$th gtt.; No. 12, $\frac{1}{10000}$th; No. 13, $\frac{1}{100000}$; No. 14, $\frac{1}{10000000}$th of a drop. These died all in my presence in, respectively 20, 26, 25½, and 20 hours, showing in addition to the first point above-mentioned, that there is no appreciable shortening of the incubation period between the second and the sixth generation. As the blood here proved to be infective in the ten-millionth part of a drop, it was now necessary to determine the limits of infectivity in the first generation.

Again five drops of putrid bullock's blood, diluted with an equal bulk of saline solution, were injected into a rabbit, No. 15, which died within 48 hours; its blood was diluted as before, and ten drops injected into each of five other rabbits, so that they received—No. 16, one-thousandth; No. 17, one hundred-thousandth; No. 18, one-millionth; No. 19, one ten-millionth; and No. 20, one hundred-millionth of a drop. No. 16 died in 24 hours, Nos. 17 and 18 died within (that is, were found dead in) 35 hours, and No. 19 within 48 hours; No. 20 was apparently unaffected; subsequently, however, it lost flesh, though continuing to feed, without any material rise in temperature or the occurrence of microphytes in the blood, and a small abscess was found at the spot of injection, which suppurated and healed spontaneously in about eighteen or twenty days, and the animal recovered.

From this series of experiments it appeared that the blood of an animal poisoned by inoculation with putrid matter, in the so-termed first generation, is infective in less than the millionth part of a drop, and the considerations mentioned below showed that it was useless attempting to investigate the limits of infectivity beyond this point; but in order to ascertain whether there occurred subsequently any appreciable shortening of the incubation period, as was considered to be the case by MM. Coze and Feltz, infection was transmitted in succession up to the tenth generation, in which the blood of a rabbit that had died septichæmic, was again diluted as before, and ten drops, each of the same dilutions as in the last-recorded experiments, injected into four other rabbits, one-thousandth, one-millionth, a ten-millionth, and a hundred-millionth of a drop respectively into rabbits Nos. 21, 22, 23, and 24, which died, No. 21 in twenty-one hours, while the other three survived upwards of twenty-seven, but died during the night within forty hours; here obviously there is no appreciable shortening of the incubation* period; but the instances recorded show how constant in the large majority of cases this period is, the quantity used for inoculation is absolutely without any influence upon it, whether some drops, or the hundred-millionth of that quantity is used; it is even seen frequently that inoculation with materially smaller quantities of the same blood produces death in a shorter period than larger quantities do, in such cases it would appear that the result can only be owing to a difference in constitutional vigour, the power of resisting infection, in the animals experimented upon. It must be remarked, too, that the apparent variation in the length of the incubation period as recorded herein, appears sometimes greater than was actually the case, for when an animal died, *e.g.*, during the night, it is recorded as having occurred "within" a certain time, whereas it may have occurred, and in some instances certainly did so, some hours previously; if the death of all the animals had been actually witnessed, this period would appear even more uniform than it does here.

In all these cases the appearances on death above mentioned were observed without material variation, and in every case there were found in the blood large numbers of a microphyte which is very characteristic and distinctive; yet although some previous writers have clearly recognised its presence, regarding it as constituting the active contagium and *materies morbi*, no accurate account of its microscopical characters has been given, nor its direct relation to the infective virulence of the blood which it infests considered.†

This microphyte is somewhat minute and in fresh preparations difficult to examine; it is a form of Bacterium, but when stained is readily recognised by the size of the cells, which is but little variable, and by its form, which is distinctive.* The numbers in which it appears in the blood in different cases, in various portions of the same blood, or even in different parts of the same preparation, are very variable; this appears to arise in some measure from a disposition to agglomerate together in places, and may be due to the tendency of the blood to coagulate. To enumerate these organisms in unstained preparations is impossible with any of our present microscopical appliances, as they cannot be sufficiently clearly distinguished, and in dried and stained preparations, not knowing the depth of the layer of blood, it can only be done by comparison with the number of the red corpuscles, obviously a rough and uncertain method. In some cases, however, they are fully ten times as numerous as the red corpuscles, and taking the number of the latter at about 5,000,000 to the cubic millimetre, we have of the former, 50,000,000, or in a drop upwards of 3,000,000,000, which corresponds as nearly as could be expected from their variable numbers and irregular distribution to the minimal quantities in which I have found the blood to be infective, viz., almost invariably in the 100 millionth of a drop;† in much smaller quantities its action is uncertain, in correspondence with the view that the microphyte does constitute the active contagium; for in that case, or if the contagium be particulate, whatever its intimate nature, to whatever degree the fluid in which it is contained may be divided by successive dilution, it is evident that any given portion may, and some one or more portions must, contain the infective particle; and hence that to determine the least quantity in which it is constantly infective is impracticable. From the dimensions of the organisms and the numbers that can be comprised in a given space,‡ it is evident that blood containing them cannot be constantly infective in the quantities stated by some observers, viz., in the trillionth of a drop or less, yet it might be so exceptionally, and consequently the original statement of Davaine may have been strictly correct, in the instances he has recorded, though the inference usually drawn from them is erroneous.

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* I have here used the term "incubation period" in the same manner as done by Davaine and others, not in its proper sense of denoting the period between infection and the first appearance of any symptoms of disturbance, but in reference to the duration of the malady between inoculation and the death of the animal; this, though not strictly correct in the proper signification of the term, may, perhaps, be excused, inasmuch both as the first occurrence of any constitutional disturbance is not well marked nor easy to recognise, in the animals the subjects of these experiments, and also as the period between the appearance of such symptoms and subsequent death is, in ordinary cases, very short, seldom exceeding a few hours.

† MM. Coze and Feltz, indeed, described and figured a microbe which they found in the cases they examined. If their figures are drawn to scale, this cannot be the same bacterium here in question, or otherwise the figures are erroneous, for they represent a form fully double in breadth that which I have obtained, and more nearly resembling the common septic ferment, *B. termo* (Cohn), than any other species with which I am acquainted. If, however, these cases were examined immediately after death, as stated, this form could not have developed, being a septic and not a pathogenic species, nor capable of multiplying or subsisting in the tissues of the living animal. It must, however, be remembered that at the time of their observations, nearly twenty years since, the available microscopical appliances were greatly inferior to what we now possess.

In regard to the origination of infection by putrid blood, in several series of experiments, I have found that during the summer and autumn months, it is generally obtained in two or three trials; in the winter I have also obtained it; but during the present winter (1882) I have failed in numerous attempts. In putrid blood, not too old, a large number of septic microphytes of many different forms may be observed; in cases when such blood proved infective, I have sometimes, though not always, been able to recognise the specific organism herein described as invariably occurring in the blood of infected animals, and apparently constituting the actual contagium of the disease, though they were never numerous; but in these cases when the blood did not prove specifically infective, I never in one instance could recognise them, often as they were sought for. Davaine states that blood taken fresh and kept at a temperature of about 38° C. for forty-eight hours,* becoming putrid, is as virulently infective as blood in the later generations of transmitted infection, in the rabbit, viz., in the hundred-millionth of a drop or less. In several experiments, made at different times and places, I found that such blood, though rapidly developing a variety of septic bacteria, was not specifically infective in any case, even when injected in quantities of some drops; neither in such blood did I ever find the specific organism, although sometimes, and in one instance most conspicuously, there were present a large number of bacteria proper, very similar in appearance to the specific organism here in question, from which in fresh or unstained preparations it is somewhat difficult to distinguish, but when stained after the usual methods they were found on measurement, to be fully double the size in breadth of the others, and with a perceptible difference in form. But few other species were observed in these cases; that which was present is the *B. termo* of Cohn, usually the first to develope in putrefying animal or vegetable matter, they very shortly disappear and are replaced by other forms. Not having succeeded during these latter experiments in getting infection, I was unable to try the effect of inoculating fresh blood from the infection, and then incubating or artificially putrefying it.

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* Its characters I have already described ("Journ. R. Micros. Soc.," 1882, vol. ii, p. 310): it is easily distinguished from *B. termo*, which it superficially resembles, by its size; being but half the breadth (0·5 μ), and by its form; the cell-wall not being constricted in the centre as in the latter species.

† In one instance I found 10 gtt. of the blood diluted 10,000,000,000 times fatally infective within about the usual period.

‡ Viz., as already shown ("Journ. Roy. Micros. Soc.," 1882, p. 311), 250,000,000,000 in a drop taken as the sixteenth part of a cub. centim.

The fact above referred to as established by MM. Coze and Feltz, and Davaine, was in the year 1872 further extended by the experiments of Drs. Sanderson and Klein,† in this country, who showed that by injection into the peritoneal cavity of an animal, either of various pathological products or, ultimately of a chemical irritant, itself free from living organisms, or even parasiticidal, and with antiseptic precautions, an inflammatory affection was induced, the exudation products of which, always abounding in micro-organisms, produced on inoculation into other animals similar symptoms, with the recurrence of the same microphytes. Further, it was thought that in this case, too, there occurred an increase in the infective virulence of the pathogenic fluids, in successive generations of transmitted infection, similarly to the case considered hereinbefore. The remarkable circumstance here shown of the origination of an infective bacterium-containing product by means of a chemical irritant, seems to have been since lost sight of in reference to its bearings on the question of the relations of micro-organisms to infective disease. The affection was first described under the title of the "Infective Products of Inflammation;" but recently a similar disease has attracted attention under the designation of Pasteur's septichæmia, a term which is decidedly a misnomer,* the blood neither in the living animal nor shortly after death, to my experience, being in anywise infective or septic. I have found in numerous experiments made since those herein recorded, some of which have lately been communicated to the Royal Society, that these two forms of disease, which may be originated by various methods, are essentially the same, inasmuch as they are interchangeable at will, merely by altering the place of injection, the pathological symptoms in both are similar, only differing in extent and severity; the serous exudation in each has a very similar character, though the subcutaneous œdema is more constantly highly coagulable than the peritoneal exudation, the period of incubation after inoculation is similar, and the micro-organisms which occur in each are identically the same, vaguely as they are characterised by recent French writers.†

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* "Bull. Acad. Méd.," Oct. 8, 1872.

† "Med. Chir. Trans.," vol. lvi, p. 345, &c.

Guinea-pigs were used in this case, healthy animals being selected; for comparative experiments young and as much as possible of the same size. To originate infection, a small quantity of a dilute solution of ammonia was injected by means of a Pravatz syringe into the peritoneal cavity of one of these animals; the water used was previously boiled, the syringe was new, rinsed out with boiling water, and the vessels employed were disinfected by heat. In subsequent experiments with infective fluids the syringes after use were treated first with boiling water, then with a strong solution of potassic permanganate, and again washed in boiling water; that these measures were sufficient to destroy infection was proved by several experimental injections with these syringes of normal saline solution, which in every instance were innocuous. I have found guinea-pigs as well suited as rabbits for experiments on this affection, and although here, as is shown, the period of incubation is somewhat less constant than in the case of Davaine's septichæmia in rabbits, above recorded, yet it was found that when infection occurred death always followed within about thirty or forty hours; some few instances occurred in which animals that had been inoculated died subsequently to this period while under observation, but in such cases it was found that this was the result of accidental causes and not of infection. In some cases where infection failed and the animal was obviously in good health, as evinced by there being no loss of weight or of appetite, it was, after the lapse of two or three weeks, used for other experiments ; it is quite contrary to my experience in this case or in that of Davaine's septichæmia, that an animal inoculated should die of septic infection after the lapse of several days or some weeks, as has been recorded by other observers. In the experiments on transmitted infection the septic fluids employed were always diluted with normal saline solution recently made and freshly boiled : in cases where moderate quantities—two or three drops or upwards—were used, equal parts of each were taken; when much smaller quantities were required, they were obtained by the method of "fractional dilution." In all cases the quantities given as having been used for injection apply to the actual quantities of the septic fluid and not to its dilution.

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* It has since been styled by Dr. R. Koch more appropriately "malignant œdema."

† M. Pasteur, however, shows that he is familiar with the microscopical characters and measurements of these organisms, "Bull. Acad. Med.," 1881, p. 97, and from this passage and other circumstances it seems to me shown that the "new disease" therein referred to as a form of rabies, is identical with Davaine's septichæmia.

Experiments.

0·3 cub. centim. of a dilute solution of ammonia was injected into the peritoneal cavity of a guinea-pig which, when examined the next morning, had apparently died some hours previously, decomposition of the viscera and abdominal wall being far advanced, destructive inflammation around the site of injection had occurred, and there was a considerable quantity of peritoneal exudation, containing red blood-corpuscles, much altered; some pus or leucocytes, mostly largely vacuolated, with a great number of Bacilli, and spores or Micrococci; the former actively mobile, *i.e.*, they possessed the power of independent movement, while the spores or Micrococci were merely affected by the Brownian movement.*

In the lower layer of the tissues of the abdominal wall were found in places several Bacilli and some few Micrococci or spores; these occurred mostly in the connective tissue and between the muscular fibres; none were found in the skin or subcutaneous connective tissue, either here or in any case of the intra-peritoneal injection of a chemical irritant; the puncture in the skin and abdominal wall by insertion of the needle of the syringe had produced but very slight, scarcely appreciable, inflammation; in some other cases the spot was not perceptible even on microscopical examination of the tissues. The occurrence of the microphytes, in the situation only here stated, in conjunction with the circumstance that the substance injected was a chemical irritant, germ-free, is important in respect to the question of their origin.

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* Micrococci, however, are sometimes actively mobile; but in many cases where moving bodies seen under the microscope are taken for these organisms they are merely the elongated cells of bacteria or Bacilli seen endwise, in "optical section" as it is termed, *i.e.*, floating perpendicularly to the cover-glass of the preparation.

These Bacilli are in width about 1·0 to 1·3 μ (0·001 to 0·0013 millim.): in length, the single cells vary up to about 4 or 5 μ (0·004 to 0·005 millim.); filaments consisting of some few of these, united end to end, and in less active movement than the individual rods, also occurred in the fluid as examined fresh under the microscope, but in dried and stained preparations these are not nearly so numerous. In dimensions these Bacilli are somewhat similar to the *B. anthracis*, but it is impossible for any careful observer to confound the two; for independently of the species here described being active, while the *B. anthracis* is invariably immobile, and though in the serous fluids the former develope into filaments of variable length, yet the segments of these show the original mature cells of about the dimensions here given, into which they again break up, *preserving their rounded ends*, and are distinctly unlike the segments which the filaments of *B. anthracis** form, as it is possible to be, for the latter are *rectangular*, pretty uniform in length, of little more than about twice the width; in cases of Anthrax, whether in the animal organism, or in artificial cultivation this formation may invariably be recognised where the cells are sufficiently developed to form filaments. In the present case blood from the heart contained no organisms, but in the spleen and peripheral portion of the kidney were found several Bacilli, though not in the other organs. In this case it is to be noted that death had occurred some, probably several, hours before examination, and decomposition was advancing with the rapidity that characterises all these cases. In other experiments where the infected animals were examined immediately after death, in no instance were any microphytes found either in the blood or organs, but only in the serous fluids and in the connective tissue as before stated.†

Of the exudation fluid in this case 0·05 cub. centim., diluted with equal parts of salt solution, was injected into guinea-pig No. 2, and 0·022 cub. centim. similarly into another, No. 3. Both these animals died in between twelve and eighteen hours with appearances similar to the first case, but less severe; the same organism occurred in the peritoneal exudation, but none elsewhere. Two other animals were also injected at the same time with respectively 0·0044 and 0·0022 cub. centim. of the same fluid diluted, but neither was appreciably affected. It thus appeared that the inflammatory exudation fluid of the first generation originating infection is fatally infective in quantities of from 0·022 cub. centim., but not in 0·0044 cub. centim. To continue the infection through subsequent generations, 0·05 cub. centim. of case No. 2 was injected into another animal, which likewise died in about thirty hours, and was examined immediately, the same hyperæmia with exudation and the occurrence of numerous organisms was found; of this serum, 0·05 cub. centim. diluted, was injected into another animal, No. 7, which survived, without being materially affected, thereby showing that the infective virulence of the fluids is variable, and that in the third generation of successive infection there is at least no gain in virulence from the first, in which it was fatal in less than half the quantity that was here without material effect. Infection being thus lost, it was again originated in another series of experiments by injecting 0·22 cub. centim. of a dilute solution of ammonia into the peritoneal cavity of another guinea-pig, which died within twenty-four hours, and was examined some time after death with very much the same appearances as in the first case recorded above, though here the serum was more deeply sanguineous and more coagulable; the same organisms were found to be present. Of this fluid 0·05 cub. centim. was injected into another animal, No. 9, without, however, producing any appreciable effect; thus again showing that in the same generation of infection, viz., here in the first, the virulence of the septic product is variable; it was therefore necessary to work with larger quantities. In a fresh series of experiments infection was again originated by the injection of a solution of ammonia into another animal, No. 10, which died in manner similar to the former. Of the exudation fluid in this case 0·22 cub. centim. diluted, was injected into No. 11, which died in between eight and nine hours after; another, No. 12, which received by injection 0·055 cub. centim., died in between twelve and twenty hours; subsequently 0·022 cub. centim. of the same fluid, of No. 11, was injected into another animal, No. 13, which remained without being materially affected, beyond, as in other cases which survived, some rise of the rectal temperature, which, however, in these animals is not sufficiently constant normally, for variations in it to give any reliable indications, unless very marked. In healthy animals I have found it fluctuate from very slight causes, such as fright, being handled, &c., between 97° F. and 101° F. Other animals which here received injections of smaller quantities than the above all remained similarly unaffected.

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* The terminal cells, however, of this form too, it must be remarked, retain their rounded ends, "the growing-point," though but few of these may be found in mature development.

† Some writers, however, in experiments upon Pasteur's septichæmia have referred to organisms found in the blood of infected cases. What the import of this is I cannot say, in the absence of a clearer diagnosis of this disease; whether some other form of specific septichæmia or septic infection had been obtained, or, in the absence of any precise statements on the subject, whether examination had been deferred until some hours after death, and septic bacteria had developed.

From the above-mentioned animal of the second generation that died infected, 0·22 cub. centim. of the serous fluid diluted was injected into another guinea-pig, No. 14, which died within four hours, but 0·22 of the exudation in it, being similarly injected into another, No. 15, of the fourth generation, this survived for twelve hours, and was found dead some hours subsequently, showing that, as judged by the period of incubation, the septic fluid was inferior in virulence to that of a prior generation. Infection was continued through another generation, the fifth, by injection of the same quantity of serum of No. 15 into another animal, No. 16, of which finally 0·2 cub. centim. was injected into No. 17 of the sixth generation in succession, which died in about the same period after inoculation, viz., four to five hours, as had the one previously of the third generation. The following successively diminishing quantities were then injected into other animals, viz.: 0·022 cub. centim., 0·0022, and 0·00022 cub. centim., and some smaller quantities still, all alike without producing any material symptom of disturbance. In every instance here the Bacilli were present in such numbers in the exudation serum from which the dilutions were made that the smallest quantities injected must have contained very many of them.*

In regard to the relation of these micro-organisms to the diseases in which they appear, it has been asserted† that they occur normally in the blood of healthy animals, and are only enabled to develop by the pathological condition caused in these cases by the injection of septic matter. As this point is of fundamental importance to the subject, to determine it experimentally, a young healthy rabbit, nearly full-grown, was killed by asphyxiation, and then placed in the incubator intact, for twenty-four hours; at the end of that time the abdomen was largely distended with gas, and putrefaction obviously well advanced. On examination there was found in blood from the heart, in rather small numbers, a Bacillus, the cells of which were of considerable width, but of no great length, nor forming long chains, mobile, and evidently in active multiplication, in some few cases apparently forming spores at one end only; there were a very few free spores, but growing cells in all stages of development; no small Micrococci nor any other species of Schizophyte, no *B. termo*, nor any one of the organisms here described as specifically distinctive of Davaine's septichæmia. In blood from the lungs and the liver the same Bacillus also occurred, as it did in the spleen, but in smaller numbers. In none of the organs or tissues examined was any other species found; whereas in cases of infection with this specific form of Davaine's septichæmia, the particular bacterium increases within certainly fifteen hours, so as to outnumber many times the red blood-corpuscles, here none were to be found in a materially longer period. Hence it seems to me to be clear that the specific bacterium of Davaine's septichæmia does not normally exist in the blood of healthy rabbits. Numerous preparations of blood from different parts were made, stained, and carefully examined, with the same result in all.

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* In the experiments here recorded "infection" was originated by the intra-peritoneal injection of a chemical irritant; in other series, by putrid matter; the results are substantially the same in both cases.

† Most recently by Professor Rossbach, "Vermehrung der Bakterian in lebenden Tiere, &c.," Ctbl. f. d. Med. Wiss., 1882, No. 5, p. 82.

The experiment was repeated with a guinea-pig, similarly killed and kept in the incubator for twenty-four hours, at the end of which time it was enormously distended with fetid gases, the blood from the heart and principal organs also contained a Bacillus in some numbers; this was similar in form to that found in the case of the rabbit, but somewhat less in size, and spore formation was more advanced. To all appearances it is identically the same as that which developes in cases of peritonitis or the so-called Pasteur's septichæmia in the guinea-pig. Whether or not it is specifically identical with the Bacillus that developes in dead healthy rabbits or in septic diseases in that animal, can only be determined by interchanging inoculations or cultivations of each growth, for though there is a slight morphological difference in the organisms of the two animals, it is clearly the fact that the same species of Bacillus, growing in different media, varies appreciably in its dimensions, as is typically exemplified in the case of the Bacillus of Anthrax.

Artificial Cultivation.

In the first trials of cultivating the organisms here in question in open tubes, both those of Davaine's and the so-called Pasteur's septichæmia, I failed to obtain conclusive results with any of the nutrient fluids employed, infusions of the flesh of different animals, and with blood serum: in the latter the bacteria of Davaine's septichæmia germinated, producing a turbidity which extended very slowly and to small extent, dying out generally after about the second day; the serum used being partially solidified by evaporation in prolonged heating, though still perfectly pellucid and not in any way coagulated. The Bacilli in the other case behaved in much the same manner in different fluids.

In the last experiments made during the winter, as I failed to again originate specific infection with Davaine's septichæmia by inoculation with putrid blood, I have been unable to repeat the attempt to cultivate these organisms. As, however, it has been stated that the microphyte of Pasteur's septichæmia has been successfully cultivated *in vacuo* in France, and it seemed probable, from the conditions under which it occurs in the animal organism, that it would thrive best in the absence of oxygen or atmospheric air, the experiments were repeated. In the former trials, where inoculations were made under antiseptic spray, both in the tubes of *bouillon* and of serum, accidental contaminations sometimes occurred. In the present experiments, inoculations were made through sterilised cotton wool, which is the only reliable method of artificial cultivation with which I am acquainted, and which I have found in numerous experiments, even with such unstable substances as blood serum, to be absolutely infallible, with moderate care in manipulation. It was communicated to me by Dr. E. Klein, and is, I believe, described by him in detail elsewhere. The infective exudation serum used for inoculation in this case was from a guinea-pig, in which it had been produced by the subcutaneous injection of a few drops of the serous fluid in infective peritonitis, artificially induced: the organisms herein were not very numerous, consisting chiefly of short rods of somewhat variable breadth, one end being frequently swollen, as in the formation of spores, distinct forms of which, however, were not apparent, though cell-rods, in a very early stage of development, were. It at first seemed as if there were here two distinct species of Bacilli present, but subsequent observations showed that they were only forms of one and the same in different stages of development. The narrower cells were, in fact, degenerating and withering, which frequently accompanies spore formation, the cell presenting a shrivelled and somewhat contorted figure instead of the usual uniformly cylindrical sharp contour. This stage is more frequently and readily seen in artificial cultivation outside the animal body. The tubes containing sterilised blood serum and beef *bouillon* being inoculated with a small particle of this fluid, were then exhausted of air, sealed, and placed in the incubator, when the next day their incipient turbidity showed that vegetation was proceeding readily; on the second they were opened, and both the serum and *bouillon*[*] were found to be full of the same species of Bacilli originally inoculated, in various stages of development, *forming numerous and distinct spores.* More than one tube of each nutritive medium was always prepared, and it was observed in those containing serum that the more solid it had become by evaporation the more slowly did the vegetation progress in it, rendering it fluid as it proceeded. On opening the tubes the strongly fetid odour caused by the development of the vegetation was remarkable and distinctive, and was observed in the case of every tube opened, both those with serum and those with *bouillon.* Their cultivation was repeated four times, in exactly similar manner, each cultivation lasting two or three days, and precisely similar appearances being observed in each. From the fifth collection, which contained the Bacillus in considerable numbers, five drops, diluted with an equal quantity of normal saline solution, were injected into the subcutaneous tissue of the abdomen of guinea-pig A, which died with the usual, though slight, symptoms of infection in about forty hours after inoculation. Another guinea-pig, B, at the same time received in like manner ten drops of the same fluid, diluted 1,000 times ($=\frac{1}{100}$ gtt. of the cultivation). This animal was perfectly unaffected, beyond a slight local and temporary irritation at the spot of injection, which, without infection, sometimes occurs with animals inoculated in the abdomen, and arises probably, as I have witnessed, from their scratching; it passed off within forty-eight hours and the animal remained unaffected. The quantity of fluid here injected must have contained many thousands at the least, and probably some millions of the Bacilli, but they do not appear capable *per se* of developing to any extent in the tissues of a healthy animal.

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[*] Blood serum offers a suitable cultivating medium for probably all pathogenic organisms; it is easily sterilised, and when once this is effectually done, it may be kept for any length of time, and while remaining perfectly translucent may be rendered by prolonged heating of any consistency required. Koch's method of gelatine culture, though most valuable for particular purposes, is not for several reasons suitable for use generally in these experiments. With the exception of *B. anthracis* most pathogenic bacteria, as distinguished from the merely septic, vegetate sparsely if at all in vegetable infusions, the attempts to cultivate them in which are generally disappointing and misleading.

From these experiments I conclude that in the affections here in question, that is, in Davaine's septichæmia in the rabbit and the so-termed Pasteur's septichæmia in the guinea-pig, there is no increase of infective virulence in the septic fluids in successive generations, either in respect to the minimal quantities required to produce fatal infection, nor as to any constant difference in the incubation period, though in the latter case this period is less constant than obtains in Davaine's septichæmia; the infectivity, too, of the inflammatory product, though not comparable in virulence to septichæmic blood, is here more variable, partly owing it may be, as Dr. Sanderson originally considered, to differences in the severity of the cases affording the infective matter, and partly also, as I have above stated, to constitutional idiosyncrasy in the animal inoculated.

With respect to the nature of the contagium and the relation of the micro-organism to the disease in which they occur, I conclude that in the first case, *i.e.*, in Davaine's septichæmia in the rabbit, all the circumstances taken into consideration, the microphyte constitutes the actual contagium, and that the numbers in which it is present in the blood, both septic (putrid) and septichæmic, clearly condition its infective virulence. Its numbers alone would not account for the difference in the incubation period—so-called—of the two cases, but the purity of the growth in the latter case, and in the other the fact of the specific bacterium being always far outnumbered by other species with which it has to contend in order to establish its growth, fully accounts for its slower development, and the consequent protraction of the incubation period.

In the second case, that of infective peritonitis, or Pasteur's septichæmia in the guinea-pig, the circumstances are materially different; in the first place, the microphyte appears to originate from within the animal organism, as shown both by the anatomical examination of the tissues, in the case of originating the disease by the injection of an antiseptic fluid; and, further, by the fact that a Bacillus, as far as can be determined, identical with the form occurring in these cases, is found to be present normally in healthy animals shortly after death, when kept at the temperature of the body. Moreover, the pathogenic matter containing these organisms when infective, is only so in incomparably larger quantities than is the case with the blood in Davaine's septichæmia, nor are cultivations of the specific organism more virulent; hence, although as shown elsewhere,* the infectivity of the pathogenic matter is destroyed by heat, I cannot conclude that in this case the microphyte is to be considered as constituting the contagium, *per se*, purely and simply as in Davaine's septichæmia; the large numbers in which it must be injected in order to develop its growth, in addition to the circumstances of its origin, seem to forbid this view, which would imply that in the one case it constituted the exciting cause of the pathological condition, to which it owed its development, that it is to be regarded in the one case as the secondary result, in the other as the efficient cause of the same thing. No doubt, however, when present in sufficient numbers, they greatly modify the characters of the pathological condition which they accompany. This, which was the opinion of Dr. Sanderson, at the time referred to, does not seem since to have received the attention it deserves, as the case of a disease in which the presence of micro-parasites originated by the injection of a germ-free chemical substance stands alone, and has the most important bearing on the relation of these organisms to disease.

In both the forms of septic infection herein referred to, the important function of the lower fungi which therein occur, is clear, but it is remarkable that, notwithstanding the numerous observations and experiments that have been made on the subject, and the fact that in both cases these affections were recognised by the chief writers thereon, as being of micro-parasitical origin, yet so little attention should have been paid to the microscopical examination and description of the organisms themselves. In the one case, indeed, the bacteria which occur had been figured and described by MM. Coze and Feltz, but imperfectly as above mentioned, and this, as far as I know, is the only attempt to describe them at all particularly; the terms in which the microphytes they mention are referred to by later French writers are most vague, while others have scarcely alluded to their presence, save in the most general manner, while recording experiments made with the object of investigating the ætiology of these diseases.

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* "Proc. Roy. Soc.," vol. xxxiv, p. 150.

In the case of Davaine's septichæmia, I have been able in all essential points, not only to confirm his original statements, but to account for them, more especially with regard to the incredibly minute quantities in which the blood in these cases is infective, statements the accuracy of which has been sometimes doubted. To emphasise this is due to the reputation of one who has done so much in many directions to advance our knowledge of micro-parasitical diseases, and indeed, it may be said that almost the only points left undetermined by this observer, or the mistakes which he made,* were those dependent upon microscopical investigations, the immense advance in the appliances of which the last few years now enables this to be done. In the other case, the so-termed Pasteur's septichæmia in the guinea-pig, the relations of the Bacillus therein occurring are not so clearly shown as in the former, yet if its characters of morphology and mobility as above mentioned had been carefully observed, neither this affection, nor, still less, the former, could for an instant have been confounded with charbon or anthrax, as was the case; nor again in this latter disease could any observer have fallen into the grave error of confounding the *B. anthracis* with either the Bacillus here in question, or the hay Bacillus, the characters above briefly described being perfectly constant and sufficiently distinctive to be at once readily recognised under even moderate magnifying power.

It is on the microscope that I have relied for determining the questions herein, and it appears to me that it is on the more assiduous use of its greatly increased powers which we now possess, that the advancement of our knowledge of these subjects depends.

The observations here recorded were commenced and chiefly performed in the Physiological Laboratory of the New Museums at Cambridge, during the last winter (1881): circumstances have delayed their final completion till lately, in the meantime an article on the same subject has been published at Berlin, by one of Dr. Koch's assistants, Dr. G. Gaffky,† who has arrived at the same conclusions mainly as myself with reference to the principal point in question; but on the other hand, the doctrine of an increase of virulence in septic fluids in successive generations has been again affirmed by Professor Rosenberger* in relation as appears to both the forms of disease here in question, without, however, giving any detailed account of the experiments on which he grounded his opinion.

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* As in regarding the specific organism of septichæmia and the putrid ferment as one and the same.

† "Experimentell erzeugte Septikämie, &c.," in "Mittheil. a. d. Kaiserl. Gesundheitsamte, &c.," Berlin, 1881.

In conclusion I have to thank the British Medical Association, through the Scientific Grants Committee, for assistance in defraying the expenses of these experiments.

Table I.—Showing the Result of the Subcutaneous Injection of Septic and Septichæmic Blood in different quantities in Rabbits.

No.	Generation.	Matter injected.	Quantity.	Result.
			gtt.	
1	1	Putrid bullock's blood dil...	3·0	Died within 40 hours.
2	2	Blood of No. 1 diluted.....	1·0	,, ,, 24 ,,
3	3	,, ,, 2 ,, ...	$\frac{1}{10}$	Died in 25 hours.
4	,,	,, ,, 2 ,, ...	$\frac{1}{100}$	,, 24 ,,
5	,,	,, ,, 2 ,, ...	$\frac{1}{1000}$	,, 25 ,,
6	,,	,, ,, 2 ,, ...	$\frac{1}{10000}$	Died within 40 hours.
7	,,	,, ,, 2 ,, ...	$\frac{1}{100000}$	,, ,, 27 ,,
8	4	,, ,, 3 ,, ...	3·0	Died in 23 hours.
9	5	,, ,, 8 ,, ...	3·0	Died within 48 hours.
10	6	,, ,, 9 ,, ...	3·0	,, ,, 20 ,,
11	7	,, ,, 10 ,, ...	$\frac{1}{100}$	Died in 20 hours.
12	,,	,, ,, 10 ,, ...	$\frac{1}{10000}$	,, 26 ,,
13	,,	,, ,, 10 ,, ...	$\frac{1}{100000}$	,, 25½ ,,
14	,,	,, ,, 10 ,, ...	$\frac{1}{100000000}$	,, 20 ,,
15	1	Putrid bullock's blood dil...	5·0	Died within 48 hours.
16	2	Blood of No. 15 diluted....	$\frac{1}{1000}$	,, ,, 24 ,,
17	,,	,, ,, 15 ,, ...	$\frac{1}{100000}$	,, ,, 35 ,,
18	,,	,, ,, 15 ,, ...	$\frac{1}{1000000}$	,, ,, 35 ,,
19	,,	,, ,, 15 ,, ...	$\frac{1}{10000000}$	,, ,, 48 ,,
20	,,	,, ,, 15 ,, ...	$\frac{1}{100000000}$	Survived, abscess formed.
Infection continued through successive generations up to the 10th.				
21	11	Blood of rabbit of 10th generation diluted.......	$\frac{1}{1000}$	Died in 21 hours.
22	,,	,, ,, ,,	$\frac{1}{1000000}$	Died after 27 hours, within 40 hours.
23	,,	,, ,, ,,	$\frac{1}{10000000}$	,, ,, ,,
24	,,	,, ,, ,,	$\frac{1}{100000000}$	,, ,, ,,

Note.—The quantities employed are here given in drops (minims), in order that they may be comparable with the experiments of others on the same subject.

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* "Ctrlb. f. d. Med. Wiss.," 1882, No. 4, p. 66.

Table II.—Showing the Result of the Intra-peritoneal Injection of different matter in Guinea-pigs.

No.	Generation.	Matter injected.	Quantity.	Result.
			c.c.	
1	1	Solution of ammonia diluted..	0·3	Died within 20 hours.
2	2	Exudation fluid of No. 1 dil...	0·05	Died in between 12 and 24 hours.
3	„	„ „ „ 1 „ ..	0·022	„ „ „
4	„	„ „ „ 1 „ ..	0·0044	Survived.
5	„	„ „ „ 1 „ ..	0·0022	„
6	3	„ „ „ 2 „ ..	0·05	Died in 30 hours.
7	4	„ „ „ 3 „ ..	0·05	Survived.
8	1	Solution of ammonia diluted..	0·22	Died within 24 hours.
9	2	Exudation fluid of No. 8 dil...	0·05	Unaffected.
10	1	Solution of ammonia diluted..	0·22	Died within 24 hours.
11	2	Exudation fluid of No. 10 dil..	0·22	Died in between 8 and 9 hours.
12	3	„ „ „ 10 „ .	0·055	Died in between 12 and 24 hours.
13	2	„ „ „ 10 „ .	0·022	Unaffected.
14	3	„ „ „ 11 „ .	0·22	Died within 4 hours.
15	4	„ „ „ 14 „ .	0·22	Died after 12 hours.
16	5	„ „ „ 15 „ .	0·22	
17	6	„ „ „ 16 „ .	0·22	Died in about 4 hours.
18	6	„ „ „ 16 „ .	0·022	Unaffected.
19	6	„ „ „ 16 „ .	0·0022	„
20	6	„ „ „ 16 „ .	0·00022	„

January 25, 1883.

THE PRESIDENT in the Chair.

The Presents received were laid on the table, and thanks ordered for them.

The following Paper was read:—

I. "On certain Definite Integrals." No. 11. By W. H. L. Russell, F.R.S. Received January 16, 1883.

The method by which integral (227) was obtained may be thus extended.

Suppose it was required to obtain

$$\int dxf\left(\frac{a+bx+cx^2+ex^3+\ldots}{a'+b'x+cx^2+e'x^2+\ldots}\right).$$

Put
$$z=\frac{a+bx+cx^2+ex^3+\ldots}{a'+b'x+c'x^2+e'x^3+\ldots},$$

$$\theta(x)=a+bx+cx^2+ex^3+\ldots,$$

$$\phi(x)=a'+b'x+c'x^2+e'x^3+\ldots,$$

then $\theta(x)=z\phi(x)$, and if (α) be any root of the equation $\theta(x)=0$, and $\theta', \theta'', \ldots \phi', \phi'', \ldots$, are the values of $\theta'x$, $\theta''x$ when $x=\alpha$, then we find by differentiating $\theta(x)=z\phi(x)$ that

$$x=\mathrm{U}_0+\mathrm{U}_1+\mathrm{U}_2\frac{z^2}{1\,.\,2}+\mathrm{U}_3\frac{z^3}{1\,.\,2\,.\,3}+\ldots$$

when
$$\mathrm{U}_0=\alpha_1,\qquad \mathrm{U}_1=\frac{\phi}{\theta'},\qquad \mathrm{U}_2=\frac{2\phi\phi'}{\theta'^2}-\frac{\theta''\phi^2}{\theta'^3},$$

$$\mathrm{U}_3=\frac{3\phi''\phi^2}{\theta'^3}-\frac{9\phi^2\phi'\theta''}{\theta'^4}+\frac{6\phi\phi'^2}{\theta'^3}-\frac{\theta'''\phi^3}{\theta'^4}+\frac{3\theta''\phi^3}{\theta'^5}.$$

Hence $\int dxf\left(\frac{a+bx+cx^2+ex^3+\ldots}{a'+b'x+c'x^2+e'x^3+\ldots}\right)$

$$=\int dz\left(\mathrm{U}_1+\mathrm{U}_2z+\mathrm{U}_3\,.\,\frac{z^2}{1\,.\,2}+\ldots\right)f(z)\quad .\quad (234).$$

The theorem applies with great facility when $f(z)=\sqrt[n]{z}$. I of course suppose that $f(z)$ can be expanded in terms of z by a converging series in other cases. The series $U_1+U_2z+\ldots$ would, I believe, be convergent in many cases, where θ' is large when compared with $\phi, \phi', \ldots$; but the subject requires further consideration.

If we put $x=0$, $z=\frac{a}{a'}$.

Hence we immediately deduce from the equation

$$x=U_0+U_1z+U_2\frac{z^2}{1.2}+\ldots \quad . \quad . \quad . \quad . \quad (235)$$

the theorem $U_0+U_1\frac{a}{a'}+\frac{U_2}{1.2}\frac{a^2}{a'^2}+\frac{U_3}{1.2.3}\frac{a^3}{a'^3}+\ldots=0.$

Since $$\int_0^1\int dx x^{n-1}=\frac{1}{n(n+1)},$$

$$\int_0^1\int dx(1+x)x^{n-1}=\frac{1.2}{n(n+1)(n+2)},$$

$$\int_0^1\int dx(1-x)x^{n-1}=\frac{1.2.3}{n(n+1)(n+2)(n+3)},$$

and, therefore, as before,

$$\int_0^1\int dx x^{n-1}\phi(1-x)$$

$$=\frac{A_0}{n(n+1)}+\frac{A_1.1.2}{n(n+1)(n+2)}+\frac{A_2.1.2.3}{n(n+1)(n+2)(n+3)}+\ldots \quad . \quad (236)$$

with precisely similar formulæ for $\int_0^1\int\int dx^3x^{n-1}\phi(1-x)$ and other like integrals.

We also have

$$\int_0^1\frac{\phi(1-x^2)dx x^{2n}}{\sqrt{1-x^2}}$$

$$=\left\{A_0+\frac{A_1}{2n+2}+\frac{A_2.1.3}{(2n+2)(2n+4)}+, \text{\&c.}\right\}\int_0^1\frac{dx x^{2n}}{\sqrt{1-x^2}} \quad . \quad (237),$$

$$\int_0^\omega\phi\left(\frac{x^2}{1+x^2}\right)\frac{dx}{(1+x^2)^n}$$

$$=\left\{A_0+\frac{A_1}{2n}+\frac{A_2.1.3}{2n(2n+2)}+, \text{\&c.}\right\}\int_0^\omega\frac{dx}{(1+x^2)^n} \quad . \quad . \quad (238).$$

It is easily seen that these investigations extend to a great multitude of what are usually called binomial integrals. It will be seen that these formulæ include a vast number of such integrals as

$$\int \frac{dx\,.\,x^{2n}}{\sqrt{a^2-x^2}\sqrt{1-x^2}} \quad . \quad . \quad (239). \qquad \int \frac{dx\,.\,x^{2n}}{\sqrt[3]{a^2-x^2}\sqrt{1-x^2}} \quad . \quad . \quad (240).$$

$$\int \frac{dx\,.\,x^{2n}}{\sqrt{a+bx^2+cx^4+\,\ldots\,+ex^{2n}}\sqrt{1-x^2}} \quad . \quad . \quad . \quad . \quad (241).$$

$$\int \frac{dx\,.\,x^{2n}}{\sqrt[4]{a+bx^2+cx^4+\,\ldots\,+ex^{2n}}\sqrt{1-x^2}} \quad . \quad . \quad . \quad . \quad (242).$$

If we have an integral of the form

$$\int d\theta\epsilon^{\mathrm{P}x}\mathrm{Q}=\epsilon^{ax}b,$$

where P and Q are functions of θ, we have by applying the symbol $\phi\frac{d}{dx}$ to the integral

$$\int d\theta\phi(\mathrm{P})\epsilon^{\mathrm{P}x}\mathrm{Q}=\phi(a)a^{x}b \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (243).$$

As an example of this

$$\int_0^{\frac{\pi}{2}} d\theta\{\phi(\cos^3\theta\epsilon^{3i\theta})\epsilon^{x\cos^3\theta\epsilon^{3i\theta}}+(\phi\cos^3\theta\epsilon^{-3i\theta})\epsilon^{x\cos^3\theta\epsilon^{-3i\theta}}\}=\pi\phi(\tfrac{1}{8})\epsilon^{\frac{x}{8}} \quad (244),$$

from whence

$$\int_0^{\frac{\pi}{2}} d\theta\epsilon^{x\cos^3\theta\cos3\theta}\frac{\cos(x\cos^3\theta\sin3\theta)+\mu\cos^3\theta\cos(x\cos^3\theta\sin3\theta-3\theta)}{1+2\mu\cos^3\theta\cos3\theta+\mu^2\cos^6\theta}$$

$$=\frac{4\pi}{\mu+8}\epsilon^{\frac{x}{8}} \quad . \quad . \quad . \quad (245).$$

In the same way we may deduce general formulæ from integrals (86), (87), (116), (118), (122), (129), (130), of the present series.

The following integrals were obtained by the use of reciprocal functions:—

$$\int_0^{\pi} d\theta\cos^r\theta\epsilon^{a\cos\theta}\cos(a\sin\theta+r\theta)=\frac{\pi}{2^r} \quad . \quad . \quad . \quad . \quad (246),$$

$$\int_0^{\pi} d\theta\cos^r\theta\,.\,\frac{\cos^r\theta-\alpha\cos(r-1)\theta}{1-2\alpha\cos\theta+\alpha^2}=\frac{\pi}{2^r} \quad . \quad . \quad . \quad . \quad (247),$$

$$\int_0^{\pi}\frac{d\theta}{(1-2\alpha\cos r\theta+\alpha^2)(1-2\beta\cos s\theta+\beta^2)}=\frac{\pi(1+\alpha^s\beta^r)}{(1-\alpha^2)(1-\beta^2)(1-\alpha^3\beta^r)}$$

$$. \quad . \quad . \quad . \quad . \quad . \quad . \quad (248),$$

where r and s are prime numbers.

$$\int_0^{\pi} d\theta\{\epsilon^{a\epsilon^{\theta i}}\phi(\cos\theta\epsilon^{\theta i})+\epsilon^{a\epsilon^{-\theta i}}\phi(\cos\theta\epsilon^{-\theta i})\}=2\pi\phi(\tfrac{1}{2}) \quad . \quad . \quad (249).$$

This last formula is derived from (246). I observe that the fundamental idea by which these integrals are obtained is given by the equation

$$\int_0^\pi (A_0+A_1\cos\theta+\ldots A_r\cos r\theta)(B_r\cos r\theta+B_{r+1}\cos(r+1)\theta+\ldots)=\frac{\pi}{2}A_rB_r.$$

This method may be much extended.

II. "Internal Reflexions in the Eye." By H. FRANK NEWALL, B.A. Communicated by Dr. M. FOSTER, Sec. R.S. Received January 18, 1883.

1. The observation I have to record first came under my notice three or four years ago. Often when working at night by the light of a candle, in a room otherwise dark, my attention was caught by a very faint light some way out of the line of direct vision. This seemed to defy nearer inspection; for the instant I turned my eyes towards it, it was gone, thus showing that there was no objective cause, but that the light was due to some internal reflexion in the eye. Later, however, I found that by keeping the eye fixed and moving the candle, the faint light could be observed at leisure, though, as far as I could then make out, never in the line of direct vision. (See however below, § 32.)

2. The best conditions soon became apparent, and I have applied two methods in later investigations: (i) One in which the eye is fixed on a spot on a dark or uniform ground whilst the candle is moved to and fro out of the line of direct vision. (ii) One in which the candle or source of light is kept fixed, whilst the eye follows the regular movement of some point, such as the end of a pencil moved by the hand.

3. The first of these methods showed that the ghost, as I may call the faint light, moved roughly speaking in a line drawn through the point of clearest vision and the candle, in direction opposed to that of the candle's motion with respect to the point of clearest vision, and with a velocity equal to that of the candle.

4. The second method showed what is practically the same thing, namely, that the line of movement of the ghost was just as described; the direction the same as that of the point of clearest vision over the field in front; the velocity apparently about double that of the point of regard.

5. In both methods the ghost merged into the candle close to the point of direct vision, and in other positions seemed about equally removed from that point with the candle.

6. Both methods lead, as I have said, to practically the same results. The ghost then is independent of the position of the eye-ball in the socket, and hence must be produced by reflexions internal to the eye, and not brought about by its external surroundings.

7. Nearer inspection of the ghost itself showed it to be an inverted image of the candle, about equal in size, very faint, and of a slightly dull bluish tint.

8. Before saying more I will describe what has proved the easiest way of *finding* the ghost. Stand opposite a uniformly dark wall in a darkened room. Direct the eyes to any point in front (*e.g.*, a mark on the wall, a pin in a curtain), and keeping the eyes fixed and being ready to perceive any appearance out of the line of direct vision without moving the eyes towards it, hold up a candle at arm's length, and move it to and fro over about two inches, on a level with the point fixed, and a little to the right or the left of it. The ghost (or rather ghosts, if *both* eyes are used) may be seen moving with a motion opposite to that of the candle on the other side of the point of direct vision.

9. The best way that I have found for *observing* the image, is to set a candle on the table about a foot from the eye, and place close in front of it a dark-coloured board two or more feet in length, and of breadth just enough to allow the flame of the candle to be seen above when the board is set up breadthwise on its edge. Now let the eye be fixed on any object which can be moved along the top edge of the board. When this point moves the eye follows it with a *regular steady* motion, which I have failed to get by any other method, such as the seemingly more simple one of letting the gaze move along the edge of the board, unhelped by a moving point. The board in shadow makes the best background for faint images, such as the "ghost," which will be found in the position above described.

10. In either of the methods described in the two last paragraphs, the motion of the candle or of the moving point should be slow, and to and fro, over a short range; but it should be continued till the image is found, for if the image rests, it makes less and less impression on the retina.

11. Since the ghost depends on the state of the accommodation of the eye, for distinctness and even visibility (see below, §§ 15–20), it follows that people with different sight may have to look for the ghost in different ways. A short-sighted person for instance, will probably not be able to see it in the middle of the field at all, and so may fail to recognise the phenomenon, unless indeed his short sight is due to too great convexity of the cornea only.

12. Beyond a mere cursory investigation of the phenomenon, I did nothing at the subject at the time; but later, having need to consult Mr. R. Liebreich, I asked him if he knew of the phenomenon. He

was at first incredulous, but being convinced when I showed him how to find the ghost, he attributed it immediately to reflexion from the fundus, and compared it with other retinal images. He wrote to me after a day or two, saying he had found a complete solution of the problem, and would like, if I had no objection, to publish an account of some experiments he had made in connexion with the subject. Later he told me he had given up the idea, and I have not seen or heard of any published account. I had, however, made observations which led me to question his theory, and I determined to make further experiments, measurements, and calculation, to test my own view that the reflexion was from the surface of the lens, and not from the fundus alone. I was at one time inclined to consider that the ghost was connected with "Sanson's images," but was driven from that idea by considerations as to the *amount* of light that could be reflected back into the eye; this could only be extremely small. (See also below, § 30.)

13. Many reasons hereinafter related make me believe that the ghost is produced in the following way:—An image of the candle flame is thrown on the retina by the crystalline lens; this image, which I will call the first retinal image, may now be considered as a source of light from which rays proceed outwards from the retina, and are refracted by the crystalline lens and cornea to a focus again outside the eye in such a way that an image of the retinal image would be formed in the place occupied by the candle. Part of these rays, however, are reflected at the various surfaces bounding the different media of the eye, and those reflected by the anterior surface of the lens, or what one should more correctly here regard as the posterior surface of the aqueous humor, are brought to a focus somewhere between the lens and the retina. The rays from this focus have not diverged much before they fall on the retina, and there form a blurred inverted image of the first retinal image. This second image is "referred" outwards, and looks as if produced by a faint source of light outside the eye, and having a definite position in space.

14. I shall speak in what follows of this imagined source of light as the "ghost," to distinguish it from the actual image of the retina which gives rise to the sensation, and which I shall call the second retinal image. The ghost is the analogue of the candle, each being the mental image corresponding to the physical image on the retina. The name ghost will suggest to the reader similar phenomena in the telescope, which, however, are produced in a different way.

15. The ghost is affected by accommodation, so that with the candle about one foot from the eye, and with the eye accommodated for a point six inches from it, the ghost does not become visible till it is 11 degrees from the centre of the field. These numbers refer to the

case of my own eyes, and will, of course, vary with the observer. I may add here that I am what would be called long-sighted, though having a very large range of accommodation.

16. Again, when the eye is focussed for a distant point, the ghost is visible right up to the middle of the field, but invisible when removed more than 17 degrees from the centre of the field.

17. Again, if the eye and candle are fixed whilst the eye is focussed for different distances, the ghost seems also to pass through regular gradations of distinctness. (See also below, § 35.)

18. Now, in the last case, it is clear that the candle can only give a clear image on the retina in one particular state of accommodation, so that generally speaking there will be a blurred first retinal image, the reflexion of which is the second image. One would then expect that the ghost would be more likely to be distinct when the candle is at the same distance from the eye as the point focussed.

19. This I have found from observation to be the case when the eye is focussed for points not nearer than two feet. When the eye is focussed for a point six inches from it, the ghost is invisible when the candle is further than one foot from the eye, and in no position of the candle does it become very distinct, but is most so when the candle is close to the eye. With so close a point of regard the radius of curvature of the anterior surface of the lens would be much diminished, and hence also its focal length when regarded as a concave mirror, so that the rays proceeding from the focus have diverged considerably more before reaching the retina to give rise to the second image than in the case when the eye is focussed for long distances.

20. Here it may be noticed that with the point of regard at arm's length, the ghost increases much in size if the candle is approached to the eye. And also if, with the eye focussed for a near point and the candle about a foot from it, the point of regard be moved gradually away from the candle across the field, then the ghost, which is not to be seen in the centre of the field, comes into sight and grows more distinct as it becomes more moved from the centre up to a certain point, beyond which it again becomes indistinct and finally disappears. The point of distinctness varies also with the state of accommodation.

21. From considerations referred to in § 18, the question arises: Are we to regard the retina in this matter as a concave mirror, or merely as a screen? An example will help to make the importance of this clear. Suppose the source of light is the sun; then if the eye is accommodated to see it distinctly, the focus of rays from the sun would fall on the retina, which in this case would act simply as a screen, receiving a distinct image of the sun, whence rays would proceed outwards again as in § 13. If, however, the eye was accommodated for near distances, then rays from the sun would be brought

to a focus at a point in front of the retina, and would diverge again from the point and form a blurred image on the retina. Now, will this blurred image be the source of light from which rays diverge, or will the retina act as a concave mirror, so that rays from the blurred image will be reflected from it so as possibly to converge again to a focus which we may regard as the source of light; or, to put the matter more briefly, is the reflexion from the retina "regular" or "irregular"?

22. I have sought for an answer to these questions in allowing the first retinal image to fall on the depression of the *fovea centralis*, where the radius of curvature is much smaller. In this case there is no perceptible alteration in the appearance of the ghost. This points to the idea that the retina acts simply as a screen and not as a concave mirror, and that we may regard the light coming from the first retinal image (whether this be clear or blurred) as diverging from the retina.

23. The facts recounted in §§ 19, 20 lead me to believe that the second image is the reflexion of the first. One more weighty argument may here be added. If the positions of the candle and ghost be noted in any one position of the eye, and the candle be moved into the apparent position of the ghost, then the ghost will be observed in the place at first occupied by the candle: that is, the first and second images of the candle on the retina are conjugate foci with respect to some reflecting surface in front of the retina.

24. The ghost is an inverted image of the candle; therefore, its physical cause, the second retinal image, must be erect, and hence an inverted image of the first retinal image of the candle. The reflecting surface is therefore concave. The possible reflecting surfaces are the posterior surface of the cornea and the anterior surface of the lens.

25. *À priori* the cornea seems least probable for two reasons:—1st. Light from the retina must pass through the lens twice before returning to the retina, and calculation shows that the rays would proceed finally, after leaving the lens, towards the retina, as if coming from a *virtual* focus *within* the lens, 2·39 millims. in front of the posterior surface. 2nd. The light proceeding outwards from the retina and reflected back by the cornea would, in cases when the first image was far from the centre of the field, be reflected on to the anterior surface of the iris, and so would not reach the retina again.

26. I have attempted to find whether there was any illumination of the iris that would correspond to this; but two observers have failed to find light on the iris, which could not be traced to other causes. The circumstances in the case were as favourable as possible, for I made use of the sun as prime source of light, so that the light was very bright and the pupil therefore much contracted. Again, if the light is intercepted by the iris in its return to the retina, the ghost would disappear part by part, the edge of the iris intercepting more

and more of the figure of the ghost: but I find, on the contrary, that it dies away gradually, getting "out of focus," as it were, as it passes from the field.

27. On the other hand, the anterior surface of the lens seems most likely to give the solution of the problem. Calculation shows that the light proceeding outwards from the first image on the retina would be, after refraction, at the posterior surface of the lens, reflexion at the anterior surface, and second refraction at the posterior surface, focussed at a point in the vitreous humor. This point, when the eye is focussed for long distances, will be about 2·4 millims. from the posterior surface of the lens: and when the eye is focussed for a near point, it will be about 1 millim. from that surface. The distance of these points from the retina certainly forms a very great difficulty in the explanation.

28. As to the brightness or rather faintness of the image, it seems, at first sight, improbable that the surface between the lens and aqueous humor should be capable of reflecting enough of the light coming from the first retinal image, to excite the retina. But the case is comparable with the appearance of "Sanson's Images," and the brightness of the ghost, as compared with the candle, might well be described as about the same as the two faint images as compared with the bright one in Sanson's phenomenon. We might expect to find some change in the brightness of the ghost when the first retinal image falls on the more opaque part of the retina at the entry of the optic nerve. But I have failed to get *certain* results. I have allowed the image of the candle to fall on the blind spot: the ghost does not lose in brightness, though the whole field of vision seemed to become brighter, in consequence, no doubt, of the greater diffusion of light through the eyeball from the image on that part. That the ghost does not seem to lose in brightness as the ground becomes more illuminated, may, perhaps, be taken as a sign of increased brightness; but nothing definite can be gained from this point towards the explanation.

29. The fact that the ghost is visible in the centre of the field (see below, § 32) shows that the reflexion cannot be from the fundus alone, that is from the fundus *direct*, on to another part of the retina, as I understood Mr. Liebreich to have imagined. This is clear, moreover, from the fact that if the fundus were the only reflector in the case, the ghost would move in the same direction as the candle. This would also be the case if the first reflexion outwards were from a surface in front of the retina instead of the retina itself.

30. If, for instance, the reflexions were entirely within the lens (as I at first thought possible) or within the aqueous humor, then the ghost and candle ought to move in the same direction, the former with greater angular velocity. In fact, the case would be comparable

with the reflexion sometimes to be seen on the scale of a reflecting galvanometer. The light reflected from the mirror is partially reflected at the surfaces of the worked glass which closes the mirror tube: and that part reflected within the glass at the surface remote from the mirror is again reflected within the glass at the surface next the mirror, so that a second and very faint image of the slit is sometimes formed on the scale. This second image moves in the same direction as the ordinary bright one, but with greater angular velocity.

31. That the ghost is not due to reflexions within the lens is also made very probable from calculations which show that in any state of accommodation the focus would be virtual and situated in the aqueous humor. That it is not due to reflexions within the aqueous humor is made more probable from the fact that I have discovered a second ghost, which is almost certainly due to this cause; it moves in the same direction as the candle, but the faintness and indistinctness of this image is too great to allow of any accurate measurements. It is very blurred, and I have never been able to bring it even approximately to a focus on the retina.

32. The wish to see the ghost, if possible, in the line of direct vision has led me to further very interesting observations. The method was suggested by an accidental sight of the ghost in bright sunlight. I was standing on the terrace at Heidelberg, and looking towards the sunset; in front of me on the other side of the ravine was the Castle, standing out dark against the bright sky beyond. As my eye wandered slowly over the outline of the buildings, my attention was caught by the ghost flitting down below in the shadow. Here were all the necessary conditions, a bright light and a dark screen. There was no difficulty in moving the eye till the ghost fell exactly on the point of direct vision. But, strange to say, the sun was still some way out of the line of direct vision; that is, instead of seeing the sun and its ghost equidistant from the centre of the field, and finally, as the eye moved further, merging into one at the centre of the field, the ghost had got to the centre first, and had moved on to the same side of the centre as the sun before they coincided.

33. Moreover the eccentricity, as I may call it, is different for my two eyes both in amount and direction. I can best describe the difference by saying that with my right eye, when the ghost was in the line of clearest vision, the sun was still to the right and above; and when the line through my eye was horizontal, the line through the sun's position and the ghost, or the point of clearest vision, was inclined at an angle of 36 degrees to the horizon, the angular distance of the sun from the ghost being 8° 32′. With my left eye, when the ghost was in the line of clearest vision, the sun was to the left on the same level, and at an angular distance from the ghost of 6° 25′.

34. This points to something anomalous in the centering of the eye surfaces. The amount and direction of the eccentricity in my eyes as shown by these means are in somewhat remarkable correspondence with those obtained by the entirely different method of Helmholtz ("Phys. Opt.," pp. 86, 87). I may add below the angles for my eyes corresponding to numbers given by Helmholtz, *loc. cit.* :—

	Light came from	
	Nasal side.	Temporal side.
Right eye	9° 27′	3° 49′
Left eye	7° 36′	7° 36′

35. One more observation of some interest I may record. With the sun as source of light and with a fixed direction of vision (I have generally taken that direction in which the ghost comes into the line of direct vision, so as to avoid the constant tendency of the eye to turn towards any object of interest in the field), the ghost passes through a series of forms as the state of accommodation alters. There are three marked forms, a horizontal bar with a near focus; a circle for a slightly more remote focus; and a vertical bar for long distance. There may be signs of a double astigmatism; but as they are the only signs that I have had of such defect in my eyesight, I have rejected this explanation in favour of the equally simple one, that we have here the focal lines and circle of least confusion such as are observed in oblique reflexion by a concave mirror.

The explanation suggested in this paper is attended with difficulties, the greatest of which is perhaps that referred to in § 27. It is, however, the one which on the whole explains best the greatest number of the facts to be accounted for. The calculations made on various points mentioned above are based on measurements given by Helmholtz for his schematic eye. It seems likely that individual variations from the normal eye should be such as to determine the visibility of the ghost; but I have found as yet no obvious relation in about fifteen cases between the power to see the ghost and the kind of sight, as defined by the ordinary terms, long and short sightedness. Two cases in which I have notably failed to show the ghost, are opposed in this respect, one having short sight, the other normal; whilst of those to whom the ghost is visible there are as many short sighted as long sighted.

III. "Note on the Absorption Spectrum of Iodine in Solution in Carbon Disulphide." By Captain Abney, R.E., F.R.S., and Lieut.-Colonel Festing, R.E. Received January 18, 1883.

In 1876, Sir John Conroy ("Proc. Roy. Soc.," vol. 25, p. 46), described the absorption spectrum of a solution of iodine in carbon disulphide. He, however, solely regarded the visible spectrum from near B to above G, about wave-length 4,000.

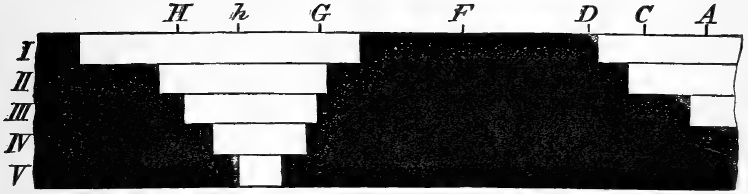
Our attention was recently directed to the absorption of this substance, and we have determined photographically the spectra for different thicknesses and strengths of the solution.

Our inquiries into the behaviour of carbon disulphide had shown us that it, as well as iodine, was transparent for rays of very low refrangibility, as was first shown by Professor Tyndall. We are not aware, however, that any investigations into the absorption at the more refrangible end of the spectrum have been published. An idea seems to be prevalent that the absorption commences in the green, and gradually extends, as the thickness or density of the solution is increased, in both directions until finally all the visible rays are extinguished, leaving the invisible rays at both ends unabsorbed.

Though one of us employed the solution before to cut off the visible portion of the spectrum ("Phil. Trans.," Part II, 1880, p. 664) in diffraction photography of the lower part of the spectrum, the use of it was abandoned owing to the opinion that was expressed that instead of the infra-red being photographed, the ultra-violet and violet of the spectrum of the next order might be that which was really impressed. Our recent experiments, however, show that no fear need have been entertained on this account, as the annexed diagram will show.*

The following thicknesses and strengths of solution were employed, being placed before the slit of the photographic spectroscope:—

	Iodine in CS_2.		Thickness.
No. I	4 per cent.	equivalent to	2 millims.
„ II	8 „	„	2 „
„ III	12 „	„	2 „
„ IV	16 „	„	2 „
„ V	32 „	„	2 „



The source of light was usually the crater of the positive pole of the electric lamp, though in two instances we were favoured with a glimpse of sunshine, which enabled us to confirm what we had before obtained with the less brilliant source of light. For Nos. I, II, and III, photographs were taken, which gave the red and infra-red spectrum, as well as the more refrangible end. With every number ordinary gelatin plates of great sensitiveness were employed as a check on the collodion plates. To the eye, the red, in each case, extends a trifle more towards D than in the photograph.

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* This diagram represents the absorption of the continuous spectrum. The bright bands in the electric light above K had power to penetrate the solutions II and III.

An attempt was made to produce the same range of photographic action in the spectrum of the thicker solutions as in the less dense, by increasing the time of exposure. The result, however, was abortive, and hence we may conclude that when any visible ray is quenched to sight by a solution of particular density, it also ceases to be photographically active.

The last ray to disappear in the blue lies somewhere close to *h*, and it will be seen that the ultra-violet rays are absorbed before the absorption touches the violet. By using a still denser solution than No. V, we were able to entirely quench the blue, both visually and photographically, while the extreme red was still visible, and the photograph taken showed no signs of absorption of the infra-red rays.

In conclusion we would remark, that by the use of this solution in a rock-salt cell and a grating, the infra-red spectrum from λ 7,600 to λ 15,200 may be photographed without the instrumental separation of the different orders of spectra; and the conditions of our climate are such that the atmospheric absorption usually, if not always, prevents a study of the solar spectrum below the greater wave-length abovenamed.

Presents, December 21, 1882.

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disease with a degree of cirrhosis which had almost destroyed the structure of the liver.*

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In 1866, with failing health, he retired from active life, and, from that time to his death, in March, 1882, lived quietly and studiously among his friends in Devonshire. The influence of his admirable personal character, and of his constancy in the patient, thoughtful striving for exact knowledge, will long survive among his many friends and pupils. In the history of medical science he will worthily rank with his brother, William Budd, a distinguished Fellow of the Society, to whom we owe, among many other good works, the first and best observations on the transmission of the typhoid infection in the intestinal excreta, and on the analogy between tuberculosis and infective fevers.—J. P.

---

* Besides the above-named works Dr. Budd published in journals, chiefly in the "Medical Gazette," several short papers and lectures, including the Gulstonian and Croonian Lectures delivered at the College of Physicians in 1843 and 1847.

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the study of the forms of cultivated plants under his charge. He was always occupied with the elaboration of careful dissections and descriptions of new and interesting genera and species of plants. He was an admirable draughtsman, and the "Traité Général de Botanique, Descriptive et Analytique," which he published in conjunction with Le Maout in 1868, is a perfect treasure-house to the student, enriched on every page with the results of his minute and careful observations and the work of his accurate pencil.

Besides this elaborate review of the whole vegetable kingdom, Decaisne published many useful contributions to detailed systematic botany. Amongst these may be mentioned his classical memoir on the *Lardizabaleæ* (1837), that on *Pomaceæ*, and the *Asclepiadeæ* and *Plantagineæ*, elaborated for De Candolle's "Prodromus."

To the period of his attachment to the herbarium of the Jardin des Plantes belong his few but standard contributions to phytogeographic literature. These include papers on the plants of Arabia Felix, Egypt, Sinai, and Timor. Morphological botany owes to him, besides the memoir on madder already mentioned, a well-known research (1841) on the development of the pollen and ovule and the structure of the stem of mistletoe (*Viscum album*), and a study on the floral organogeny of the pear (1857), the excellence of which shows how much Decaisne could have done in this branch of botany had his circumstances and leisure allowed him to devote more attention to it.

In 1842, on the death of Guillemin, he was associated with Adolphe Brongniart in the editorship of the botanical part of the "Annales des Sciences Naturelles," and on the death of the latter became sole editor. In 1847 he was elected a member of the Institute, taking the place of Dutrochet. In 1864 he was President of the Académie des Sciences; and in 1877 was elected a Foreign Member of the Royal Society. He was one of the principal founders of the Botanical Society of France in 1854, and as long as health allowed was an assiduous attendant at its meetings.

The note of Decaisne's scientific work was patient and laborious observation of facts. He was one of those whose life was spent in making sure the foundations of taxonomic botany, and every subsequent worker will build the more securely for what Decaisne has done. That his mental character was essentially precise and matter-of-fact is merely what we should expect from the direction of his work. From this no doubt it followed that the doctrines of evolution, which in England and Germany have given a new impulse to biological study, interested him little. Not that his mind was wanting in flexibility to new ideas: he warmly supported the investigations made by Bornet in confirmation of Schwendener's theory as to the nature of lichens—a view which has met with much opposition from the older botanists who are, for the most part, unwilling to abandon the conception of lichens as autonomous organisms. His method of work, partly no doubt cramped by the inroads on his time of official duties, made him more and more disinclined to generalise from large series of facts. In striving for minute accuracy he, especially in his later work, somewhat lost the gift of seeing things in a larger perspective.

He was never married, and to his friend Bornet fell the lot of watching his last moments.—W. T. T. D.

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Indian Coast, he entered upon a series of optical investigations, and devised a reciprocating light, which was fixed in the lighthouse erected at Madras in 1838, after his designs.

Before this he had translated Vicat on "Cements," enriching that valuable work with the results of many original experiments, and he was elected a Fellow of the Royal Society in 1837.

He was next appointed to reorganise the Madras Mint, which he did with great skill and vigour. When remodelling the machinery he invented a machine for automatically weighing and assorting blanks, which gained an award at the Exhibition of 1851.

After a period of some twenty years in charge of the Madras, and latterly of the Calcutta, Mint, Colonel Smith returned to England, and, after acting for some time as Consulting Engineer to various Indian Irrigation Companies, became Chairman of the Madras Railway Company, a position which he held until the close of his life.

During late years his energies were directed chiefly to the consideration of the intricate questions of political economy connected with currency, &c. A careful study of the subject for many years and a deep interest in India led him to propose a remedy for the evils caused by the depreciation of silver in that country.

He brought forward his views with great earnestness and disinterestedness, and though the question has been forced aside by more pressing matters, his proposals have met with the approval of some of the leading political economists of the day, and it is hoped that by their fulfilment at some future time many millions may be saved to the revenues of India.

His knowledge of these and of kindred subjects led to his employment by the Government to make reports in conjunction with the late Professor Graham, F.R.S., upon questions of mintage, &c., and he was also appointed to attend the International Monetary Conference held at Paris in 1865.

Colonel Smith's active mind and varied talents led him to take an interest in many other subjects. He was a member of several societies and the author of some works and several papers on various scientific matters.

In private life he was an humble Christian man. His aid as a wise and sympathising counsellor was widely sought, and his loss is deeply felt by a large number of friends. P. S.

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